THURSDAY, FEBRUARY 20, 1890.

THE PHYSICS AND CHEMISTRY OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Exploring Voyage of H.M.S. "Challenger," 1873-76. Physics and Chemistry, Vol. II. (Published by Order of Her Majesty's Government, 1889.)

THE second volume of the Report on the Physics and Chemistry of the *Challenger* Expedition has been published, and contains matter of very great interest.

The first paper is on the compressibility of water, by Prof. Tait. He has used Amagat's "manomètre à pistons libres."

"The principle on which the instrument works is the same as that of the Manomètre Desgoffes—a sort of inverse of that of the well-known Bramah Press. In the British instrument, pistons of very different sectional area are subjected to the same pressure (that of one mass of liquid), and the total thrust on each is, of course, proportional to its section. In the French instrument, the pistons are subjected to *equal* total thrusts, being exposed respectively to fluid pressures which are inversely proportional to their sections. The British instrument is employed for the purpose of overcoming great resistances by means of moderate forces; the French, for that of measuring great pressures in terms of small and easily measurable pressures."

By means of the instrument from his description of which the above is an extract (p. 21), Prof. Tait has determined the compressibilities of cistern water, sea water, and solutions of common salt up to pressures of 450 atmospheres, and for a range of temperature extending from 0° to 15° C. The results may be briefly summed up as follows.

The average compressibility of fresh water at 0° C. and at low pressures is 520×10^{-7} per atmosphere. The compressibility is a minimum at 60° C. Both the compressibility and the temperature at which the minimum occurs are lowered by pressure. The average compressibility for a pressure of 456·9 atmospheres is 478×10^{-7} per atmosphere, and the temperature of minimum compressibility is about 30° C. The average compressibility of sea water is about 0·92 of that of fresh water. The point of minimum compressibility is about 56° C. at atmospheric pressure.

At 0° C. the average compressibility of water per atmosphere may be expressed by the formula $0{\cdot}00186/(36+p)$, where p is the pressure in tons per square inch. The compressibility of solutions of NaCl, containing s parts of salt to 100 of water, is given by the formula

$$0{\cdot}00186/(36+p+s).$$

The depth of a sea about six miles deep is reduced by 620 feet by compression. If the ocean were incompressible, the level of the surface would be 116 feet higher than it is at present, and about two million square miles of land would be submerged. Finally, the maximum density-point of water is lowered by about 3° C. by an additional pressure of 150 atmospheres, and the temperature of maximum density coincides with the freezing-point at $-2°{\cdot}4$ C. under a pressure of 2·14 tons per square inch.

It will be seen from this brief recapitulation of his results that Prof. Tait has carried through a very difficult research with success, and has made substantial additions to our knowledge. It may therefore appear ungracious to criticize points which do not touch the essence of the investigation, but it is impossible to read the Report without feeling that, in some respects, it falls short of the standard of classical perfection which ought to be attained in papers published at the national expense to illustrate a great national research.

In the first place, the C.G.S. system is entirely ignored. As the compressibilities are measured *per atmosphere*, this is, so far, not of importance; but in the formulæ quoted above, which express the compressibility per atmosphere, terms occur in which the pressures are measured in tons per square inch. The units are thus mixed, and though the requisite data for conversion into atmospheres are supplied, there is no doubt that foreigners will have some difficulty in interpreting the results.

Again, though we cannot but admire the scrupulous honesty with which he tells the tale, some annoyance may justly be felt that a paper should go forth to the world in a publication intended to mark the highest level to which British science has attained, marred by the confession that the author—who deservedly holds a place in the very foremost ranks of British physicists—had never heard of Van der Waals' work on the continuity of the liquid and gaseous states till the end of the year 1888.

Van der Waals' investigation was published in Dutch in 1873. In spite of the disadvantage due to the language in which it was written, its importance was at once recognized. Clerk-Maxwell gave a long account of it in NATURE in 1874 (vol. x. p. 477). He returned to the subject in a lecture delivered before the Chemical Society on February 18, 1875, and reported in full in NATURE (vol. xi. p. 357). After indicating what he considered to be the weak points of Van der Waals' theory, he added that nevertheless "his attack on this difficult question is so able and so brave, that it cannot fail to give a notable impulse to molecular science. It has certainly directed the attention of more than one inquirer to the study of the Low-Dutch language in which it is written." Maxwell again referred to Van der Waals in his articles on "Atom" and "Capillary Action," published in the "Encyclopædia Britannica" in 1875 and 1876. So important was the theory considered, that, although it was then four years old, twelve pages were devoted to it in the first number of the "Beiblätter" to *Poggendorff's Annalen* (1877). O. E. Meyer discussed it in his "Kinetische Theorie der Gase" in the same year. It is described in modern German text-books, such as Rühlmann's "Handbuch der Mechanischen Wärmetheorie," and Winkelmann's edition of Graham-Otto's "Lehrbuch der Chemie," both published in 1885. It was translated in full into German by Dr. Roth in 1881, and an English translation by Prof. Threlfall, of the University of Sydney, is about to be published by the Physical Society of London.

In spite of all this, the author of the Report we are discussing informs us, in an addendum dated August 8, 1888, that only a few days before he had been told by a visitor to his laboratory "that one of Van der Waals' papers (he did not know which, but thought it was a recent one)

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contains an elaborate study of the molecular pressure in fluids"; and a few lines further down he refers to "Van der Waals' memoir 'On the Continuity of the Gaseous and Liquid States,' which I have just rapidly perused in a German translation."

In view of the fact that Prof. Tait published a book on "Heat" in 1884, these statements are so astonishing that his interview with the visitor from whom he heard of Van der Waals can only be described, in the words of Mr. Montague Tigg when he discovered that Martin Chuzzlewit was in the next box in the pawn-shop, as "one of the most tremendous meetings in Ancient or Modern History."

Other indications of a lack of acquaintance with what has been done by others are not wanting. Taking $p(v - a)$ = constant, as the equation to the isothermal of a gas, and assuming that it applies approximately to a liquid, the author concludes "that water [at 0° C.] can be compressed to somewhat less than three-fourths of its original bulk, but not further." He adds that "the whole of this speculation is of the roughest character," but makes no reference to the converging lines of evidence which indicate that liquids could be compressed to from 0·2 to 0·3 of their bulk at ordinary temperatures and pressures. The numbers which lead to this conclusion are frequently in good accord, whether they are deduced from direct observation on the specific inductive capacities or the refractive indices of the liquids themselves, or from those of their vapours, or from the molecular volumes of the elements of which they are composed. The latter, however, as calculated in the few cases he discussed from Van der Waals' theory, are larger, except in the case of hydrogen, than the corresponding numbers obtained from optical or electrical measurements. Van der Waals did not deal with water-vapour, but if we use the molecular volumes for H_2 and air obtained by means of O. Meyer's modification of his theory, and take the molecular volumes of air and O_2 as identical (an assumption which will certainly make the result too large), we obtain the following values:—

Volume of the Matter in the Unit Volume of Water under Standard Conditions.

Deduced from observations on the refractive index of liquid water (L. Lorentz)	0·2061.
Deduced from observations on the refractive index of water-vapour (L. Lorentz)	0·2068.
Deduced from the molecular volumes of H_2 and O_2 obtained from refractive index or specific inductive capacity	0·23.
Deduced from the molecular volumes of H_2 and air given by Van der Waals' theory	0·33.

Prof. Tait's value is 0·717. It is certainly unfortunate that a number so widely divergent from the results of a whole literature of optical, electrical, and thermal researches should be published in a *Challenger* Report without any reference to the discrepancy. It is still more unfortunate that in discussing the theory on which this result is based the opinion should be registered that "the quantity a [in the formula $p(v - a)$ = constant] obviously denotes the ultimate volume" (p. 48). This was published sixteen years after Van der Waals had given reasons for believing that a (or, as he calls it, b) is four times the ultimate volume, and twelve years after O. Meyer had argued that the multiplier ought to be increased to $4\sqrt{2}$. The best theories on the subject are no doubt tentative, their agreement with facts is imperfect, but it is established beyond the possibility of doubt that the constant in question need not have the meaning which is here said to be obvious.

Two papers in which the compressibilities of solutions of NaCl are discussed had appeared in *Wiedemann's Annalen* some little time before the conclusion of Prof. Tait's work. Röntgen and Schneider (*Wied. Ann.*, xxix. 165, 1886) determined the relative compressibilities of water and of a number of different salt-solutions, and Schumann (*Wied. Ann.*, xxxi. 14, May 1887) gave absolute measures. Both researches were carried on at low pressures only, but they are interesting in their relation to Prof. Tait's conclusions, inasmuch as his compressibilities at low pressures are obtained (as he fully explains) by an extrapolation, and it is therefore desirable to compare them with the values given by direct observation.

In the following table the compressibilities obtained by Schumann for solutions containing given percentages of NaCl (*i.e.* parts of salt to 100 of solution) are compared with the values deduced from Prof. Tait's formula:—

Percentage.	Compressibility per atmosphere $\times 10^6$.	
	Schumann.	Tait.
0	50·3	52·0
5	45·5	45·1
10	39·7	39·5
15	34·8	34·6
20	30·6	30·5
25	25·8	26·8

It is to be observed that the number 50·3 is assumed by Schumann from Grassi, and that it was employed in experiments made with water, for determining the effect of pressure on the internal volume of the piezometers. If it had been replaced by Prof. Tait's value, the close agreement between the results for mean percentages would be destroyed. Schumann also obtains maxima of compressibility for low percentages of certain salts, though he seems very doubtful about the validity of these results. We have no intention of entering into a detailed discussion of his work which certainly appears to require confirmation, but there is no doubt that nobody could have made a critical comparison between his own experiments and those of Schumann so well as Prof. Tait, when he had the whole subject at his fingers' ends. It is thus a real loss to science when a man of his great ability ignores an investigation published nearly a year before the date of his own paper.

The form of the formula given by Prof. Tait for the compressibility of salt-solutions is closely analogous to that deduced from theory by Prof. J. J. Thomson in his "Applications of Dynamics to Physics and Chemistry" (p. 184). He shows that if k' is the compressibility of water, and P is the internal pressure due to the solution of a salt, the compressibility of the solution is $k'/(1 + Pk')$. If then we put $k' = 0{\cdot}00186/(36 + p)$, Prof. Tait's formula for a salt-solution becomes $k'/\left\{1 + k'\frac{s}{0{\cdot}00186}\right\}$, which, since P is proportional to very similar to J. J. Thomson's

expression, and would be identical with it if $P = s/0{\cdot}00186$ atmospheres. In that case the internal pressure due to the salt in a solution containing 20 parts of salt to 100 of water would be about the same as the internal pressure in pure water as given by Van der Waals. If, however, we attempt to apply van 't Hoff's theory of the pressure due to dissolved substances, we find, as in the examples quoted in the "Applications" (*loc. cit.*), that the observed values of Pk' are many times greater than those given by calculation.

The second Report, by Mr. Buchan, on "Atmospheric Circulation," of which we shall give some account in a future number, is rather a treatise on meteorology than a simple discussion of the *Challenger* observations. All the data, other than those derived from the expedition (which have been previously published), are set forth, and a vast collection of meteorological facts from all parts of the world is utilized.

It would be impossible to attempt to discuss Mr. Buchan's conclusions in detail, but one may be selected as an example. Twenty-six thunderstorms occurred at sea during the voyage, and of these only four took place between 8 a.m. and 10 p.m. Nineteen occurred when the ship was near the land, and these were pretty evenly distributed throughout the twenty-four hours. Over land thunderstorms are most frequent during the day. At sea thunderstorms are nocturnal, and occur chiefly during the morning minimum of pressure.

"Over the land the maximum of thunderstorms occurs during the hours of the day when temperature is the highest, but over the open sea during those hours when temperature is lowest. The great majority of thunderstorms over the land thus occur during the part of the day when the ascensional movement of the air from the heated surface of the ground takes place" (p. 32).

These facts furnish Mr. Buchan with an interesting suggestion as to the cause of these differences:—

"As regards thunderstorms over the land surfaces of the globe, the disturbance of atmospheric equilibrium, resulting in ascending and descending currents, is brought about mainly by the superheating of the surface and thence of the lowermost strata of the air. But as regards the open sea, this mode of disturbing the atmospheric equilibrium cannot take place, inasmuch as the influence of solar radiation is only to raise the temperature of the surface of the sea not more than a degree. Hence it is probable that the disturbance of the equilibrium of the atmosphere, in the case of thunderstorms over the open sea, is brought about by the cooling of the higher strata of the atmosphere by terrestrial radiation" (p. 34).

There can be little doubt that Mr. Murray is right in thinking that Mr. Buchan's Report will be a standard work of reference for many years to come.

The third Report, by Commander Creak, is on the Magnetical Results of the voyage. As the author has himself described the main results of his investigations in the pages of NATURE, it is unnecessary to do more than refer to its most salient features. We have two, and only two criticisms to make. Commander Creak has employed the British unit of force, and his paper will therefore be used with less comfort and ease by most magneticians than if he had employed the C.G.S. system. Perhaps, however, as an Admiralty official he felt bound to adhere to the traditions of his office. Again, we think that he has been rather too modest in the amount of space he has claimed. Like Mr. Buchan, he has used information from many sources which are not, or at all events are not stated to be, generally accessible. These he has employed in determining the rates of secular change during the last 40 years all over the globe. It would have been interesting if means could have been devised for showing not merely the results of this investigation but the data on which they are based. Again, the map in which the direction of motion—eastward or westward—of the north pole of the needle is graphically shown for the period considered would have been more valuable if the magnitudes of the mean annual motion at different places had been added. This has, in fact, been done in a recent German work on the same subject.

But if we are inclined to wish that Commander Creak had claimed a larger share of space and given more details, in what he has done he has gone beyond any previous writer. His work is of the highest importance as introducing a novel view of the causes of secular magnetic change, and in connecting it with certain definite localities.

Mr. Buchan has furnished us with new meteorological maps. Commander Creak has prepared new magnetic maps, which enable us to institute a comparison between the magnetic state of the globe in 1880 and its condition when Sabine portrayed it for an epoch some 40 years earlier. The positions of the magnetic poles and foci of maximum intensity do not appear to have altered. The secular change is associated, not with these, but with four points, towards two of which the north pole of the needle is veering, and from two of which it is apparently being repelled. The points of increasing attraction on the north-seeking pole are to the south of Cape Horn and in the south of China; the foci of diminishing attraction are in the Gulf of Guinea and near the north magnetic pole in Canada. The existence of this last focus is more or less hypothetical, but in the case of the other three the various magnetic elements concur in indicating the same neighbourhood as the centre of change. Thus not only is the secular variation of the declination of opposite signs to the east and west of these points, but the increase of the downward attraction on the north pole of the needle is a maximum near Cape Horn and in China, and a minimum (*i.e.* a maximum decrease) in the Bight of Benin.

Again the annual change of horizontal force is very small near Cape Horn, but it is decreasing in South America, and the rate of decrease is a maximum at a point between Valparaiso and Monte Video. These are precisely the kind of results which would follow from the gradual production of a subsidiary centre of relative attraction on the north-seeking pole of the magnet near Cape Horn. The real existence of the Gulf of Guinea centre is similarly confirmed. Commander Creak cautiously abstains from theorizing on these remarkable facts, but there can be no doubt that he is right in thinking that they must lead us to look for the chief causes of secular variation within the globe rather than in solar or extra-terrestrial influences. His paper will be a point of new departure in the science of terrestrial magnetism.

It will be seen from what has been said that the three Reports which have been discussed are written with a wider scope than the mere discussion of the observations

made during the voyage of the *Challenger*. Prof. Tait's paper has indeed little connection with the work of the Expedition. Mr. Buchan and Commander Creak have worked up an immense amount of matter derived from other sources.

The records of the *Challenger* have not only added facts of great importance to our stock of knowledge; but have been, as it were, nuclei round which a host of other observations have crystallized into orderly arrangement. Each one of the authors has made a step forward. Prof. Tait has extended the range of pressure over which compressibilities have been measured. Mr. Buchan has attacked the diurnal climatology of the ocean. Commander Creak has given a new turn to our ideas on the secular change of terrestrial magnetism. It is only to be regretted that the exclusive use of British systems of measurement, and the other blemishes to which we have felt compelled to refer, give a certain insular appearance and character to a work of world-wide interest.

The Report on the Rock-Specimens collected on Oceanic Islands, by Prof. A. Renard, consists of 180 pages, well illustrated by woodcuts and seven maps, and constitutes a very important part of the petrology of the *Challenger* Expedition. The account of the rocks of St. Paul's from the pen of Prof. Renard has already appeared in Vol. II. (Narrative), Appendix B, of the *Challenger* Reports, and we are glad to learn from the preface to the volume now before us that the "Report on Deep-Sea Deposits" which has been so long looked for by geologists, is to be issued next month.

Mr. Murray is to be congratulated on having secured the services of so able a mineralogist and petrographer as Prof. Renard to describe the rocks brought home by the Expedition. Most of these descriptions have already appeared in the *Bulletin of the Musée Royal d'Histoire Naturelle de Belgique;* but English geologists will be glad to see them collected together and published in their own language, and in a convenient form for reference.

Prof. Renard explains in his opening remarks the grounds for publishing this account of the rock-specimens collected on the oceanic islands by the officers of the *Challenger* Expedition :—

"Mr. Murray had discovered that loose volcanic materials played a very large part in the formation of the deposits of the deep sea, and it was considered desirable to institute a comparison between these and the products of the same origin in volcanic islands situated in or on the borders of the great ocean basins."

It is at the same time admitted, by the editor of the volume, that Prof. Renard's lithological and mineralogical descriptions must be regarded rather as contributions to the geology of the islands visited, than as supplying full and descriptive discussions of the subject.

"The necessities of the voyage, bad weather, or the difficulties of the exploration, prevented, in many cases, the naturalists from passing more than an hour or two on shore; they were thus unable to give any detailed account of the stratigraphical relations, and the collections of hand-specimens were sometimes limited to those rocks situated near the coast."

In the case of Tenerife, of which we have such full descriptions in the writings of Von Fritsch and Reiss, and of Sauer; in that of the Cape de Verde Islands, the rocks of which have been carefully studied by Dölter; and of Fernando Noronha, which has been surveyed and its rocks admirably described by Profs. Branner and Williams, it is obvious that the description of the specimens placed in the hands of Prof. Renard can only be regarded as supplementary to the fuller and more comprehensive accounts of the geology of the islands which we already possess. But in the case of some of the smaller islands, like Tristan da Cunha, Marion Island, and Heard Island, the notes in the present Report constitute almost the only materials which exist for judging of their geological constitution and structure. In the case of the Island of St. Thomas, in the West Indies; of Kandavu, in Fiji; of the volcano of Goonong Api, in the Banda Islands; of the volcano of Ternate, and of several islands in the Philippine Group, Prof. Renard has taken the opportunity afforded to him by the receipt of interesting specimens casually collected, to discuss points of considerable mineralogical and geological interest.

Quite apart from their connection with certain localities, these very careful notes of Prof. Renard on peculiarities exhibited by rock-forming minerals are of much value to geologists; and so also are the series of analyses of these rock-specimens, made, evidently with great care, by Dr. Klement.

So many of the islands visited by the *Challenger* were previously touched at by the *Beagle*, on board of which Charles Darwin was acting as naturalist, that it is impossible to avoid comparing the work before us with that author's classical memoir, "Geological Observations on the Volcanic Islands," which was published in 1844 and re-issued in 1876. In spite of the improvements of our petrographical methods during the half-century, which has witnessed the application of the microscope to the study of rocks, it is very interesting to see how often observations made by Darwin, aided by that great pioneer in crystallographic research, Prof. W. H. Miller of Cambridge, are confirmed by the painstaking labours of Prof. Renard. There is, perhaps, some danger, at the present day, that the facilities afforded for the microscopic study of rocks, by the aid of transparent sections, should lead geologists and mineralogists to despise, or to regard as of small value, the observations made without such aid. To those who entertain such an idea, it will be instructive to see how Darwin and Miller by the aid of pocket-lens, knife-blade, and magnet, were often able to form an appreciation of the mineralogical constitution of rocks, which has been very largely confirmed by the application of the more refined methods of the present day.

The discussion of great geological problems, which, as treated by Darwin in 1844, contributed so largely to the interest excited by his book, have of course not come within the scope of the work undertaken by Prof. Renard. The particular varieties of volcanic rocks in Ascension, which Darwin found to illustrate in so striking a manner the origin of foliation in the crystalline schists, do not seem to have been among those collected by the officers of the *Challenger*. But as an important contribution to micropetrography, the work of Prof. Renard is of the highest value, as might indeed have been anticipated from the well-proved skill and acumen of the author in this interesting branch of scientific research.

THE HUMAN FOOT.

The Human Foot: its Form and Structure, Functions and Clothing. By Thos. S. Ellis. (London: J. and A. Churchill, 1889.)

THIS book is an endeavour on the part of a practical surgeon to explain the mechanical construction of the human foot, and from this basis to show the principles on which boots and shoes ought to be constructed. Although written in a popular form, and intended for the instruction of the public, it is treated in a scientific spirit by one who is competent, on the ground of anatomical knowledge, to discuss the subject. Mr. Ellis was led to give special attention to the mechanism of the foot owing to one of his feet having been accidentally injured; and his recovery from lameness was due to the independent study which he was obliged to give to the structure of the foot in relation to its functions.

The earlier pages of the book are occupied by a short but clearly-written description of the form of the foot, and of so much of its anatomy as is needed to explain its mechanism. In the course of this description the author points out that the two feet are to be considered together, not as if they were two independent pedestals, or plinths, supporting the lower limbs and body, but as the two halves of one pedestal or plinth, the divisions of which are separated from each other. He recognizes the inner margin of the foot in its front or expanded part as forming a straight line, whilst the outer margin forms a bold curve, and acts as a sort of buttress to the main structure of the foot. The inner margin also is elevated to form the arch of the instep. He refers to Prof. Meyer's well-known line continued backwards from the mid-line of the great toe through a central point of the heel which follows the line of the long flexor of the great toe, and states that this line corresponds with the highest part of the ridge on the dorsum or upper surface of the foot, which indicates the course of the long extensor of the great toe.

The importance of the great toe in the construction of the foot is dwelt upon by Mr. Ellis. He shows that, when the foot is used as the basis from which the body is to be propelled forwards in the act of progression, the great toe leaves its fellows and passes towards the mesial plane between the two feet, but that it is not bent in so doing. On the other hand, the smaller toes, whilst being pressed against the ground, become bent, and the phalangeal joints are lifted upwards.

The relative length of the great and second toes is also discussed. As is well known, in many of the statues of ancient art the second toe is modelled somewhat longer than the great toe, but as a rule in nature itself the great toe is the longer. Exceptions, however, occasionally occur. The writer of this notice has now before him the casts of two well-formed feet, from a man and a woman, in both of which the second toe projects beyond the great toe. He has also in his possession casts of the feet of several of the aborigines of Australia, taken under the superintendence of Prof. Anderson Stuart, of the University of Sydney, in which interesting variations in the relative length of these toes may be seen. In a man and one woman the great toe is longer than the second; in another woman the second toe in the right foot is longer than the first, but in the left foot the opposite is the case. In an Australian boy, aged 4, in the right foot the great toe is slightly the longer, but in the left foot the second toe has the advantage. In none of these Australians had the feet ever worn shoes, so that the variation in the length of these toes is natural, and not produced by artificial means. It would appear, therefore—as was shown several years ago by Prof. Ecker, of Freiburg, and by a writer in NATURE, to be the case in the hand with the ring and index finger—that variations in relative length may occur, not only in different individuals, but in opposite limbs in the same person.

The author then discusses the movements at the joints of the foot and the action of the muscles; more especially when the heel is raised and the foot rests on tip-toe as in the movements of progression. He regards the long flexor of the hallux as exercising a bow-string or tie-rod influence, bracing up the arch and diminishing the distance between the heel and the great toe. Hence the exercise of dancing is one of the most important means of promoting and maintaining the strength of the foot. As regards the act of walking, Mr. Ellis contends that what he calls the "four-square position," in which the inner borders of the great toes are retained almost parallel to each other, is that which is most conducive to steady and continuous progression, for the joints and muscles of the foot obtain through it momentary rest in the intervals between the steps. He condemns the military position, with the toes turned outwards, both in standing and walking, as much more fatiguing, by keeping the muscles and joints in a constant strain. The condition of "flat-foot" ought never to arise if the tie-rod action of the long flexor muscles of the toes be sufficiently exercised by frequent springing of the foot to tip-toe, such as takes place in the act of dancing.

The author applies the anatomical principles which he has expounded to the construction of stockings and shoes. He holds that quite as much mischief is done to the feet by wearing ill-made socks as badly-shaped shoes. He considers that a stocking with a separate stall for the great toe is always desirable, but that a straight inside line is imperative. To obtain a properly fitting boot it is necessary, in addition to the measures of length and girth, to have the contour lines of the foot, and to obtain these the author has devised a foot-stand or pedistat, a description and figure of which are given in the book. From these measures a last can be made which conforms to the shape of the foot throughout as it stands on a level surface.

We recommend the perusal of this book to all who are interested in the mechanism of the foot, and in obtaining for it well-fitting socks and shoes; and we do so with the more confidence as the author had obviously passed through a painful experience before he had satisfied himself of the principles which ought to be attended to in the construction of its clothing.

OUR BOOK SHELF.

Das australische Florenelement in Europa. Von Dr. Constantin Freiherr von Ettingshausen. Pp. 10. Tab. I. (Graz: Leuschner and Lubensky, 1890.)

THIS is a defence of the identification of fossil plants from the Tertiary beds of Europe, chiefly from Austria and

Hungary, with existing Australian genera. Baron Ettingshausen himself is largely responsible for these identifications, which have been questioned "by certain critics insufficiently acquainted with the subject." He claims that he was supported in his views by such eminent palæontologists as Franz Unger and Oswald Heer. It is now some years since Unger published his sensational "Neuholland in Europa." In this little work almost every one of a set of Eocene fossil plants is identified with some essentially Australian genus, and often, we should add, on the very slenderest of material. The late Mr. G. Bentham, who, as is well known, handled and described every Australian plant of which specimens had been collected up to his time, disputed the correctness of the identifications, and endeavoured to prove that the remains might well be those of genera still found in the northern hemisphere; yet Baron Ettingshausen gives us to understand that Mr. Bentham confirmed his determination of a European fossil leaf as belonging to the genus *Dryandra*.

Quite recently the Marquis de Saporta has attacked Baron Ettingshausen's position, and the present pamphlet may be regarded as a reply. The author concludes with the statement that, to prevent misunderstanding, he wishes it to be known that any objections or criticisms will meet with no response from him, because he is convinced of the accuracy of his "facts," and his time is too valuable to enter upon superfluous discussion. Without discussing his "facts" one by one, and without actually denying their accuracy, we may say that the illustrations given are by no means convincing, as most botanists who have worked many years in herbaria on plants from all parts of the world, we believe, will agree. Few persons probably have paid so much attention to the venation and forms of leaves as Baron Ettingshausen, yet we find none of his determinations absolutely beyond doubt. So far as we are aware, not a single fruit of *Eucalyptus* or of the assumed *Proteaceæ* has been discovered in the European Tertiary formations. As to his leaves of *Eucalyptus*, they might be matched in the genus *Eugenia*, and we see no reason why any of the others are necessarily remains of species of Australian genera. W. B. H.

Is the Copernican System of Astronomy True? By W. S. Cassedy. (Standard Publishing Co., Kittanning, Pa., 1888.)

An astronomer nowadays would find it a hard task to bring forth any facts which would throw doubt upon the truth of the Copernican theory, but it appears that there are still people amongst us who are bold enough to attack the strongholds of astronomy. Such attempts are always hopeless failures, and the one under notice is no exception. It is, indeed, doubtful whether the author knows what is meant by the Copernican system, for he goes so far as to suggest that the known diameter of the earth's orbit (assuming that it exists) should be used as a base-line for determining the distance of the sun! He also states that he has "found by experiment" that similar right-angled triangles have sides proportionate in length, though it is only fair to say that he is aware of the existence of the first book of Euclid, if not of the sixth.

We have already said enough to show that the book need not be considered seriously; but we cannot refrain from stating that the author, by sighting the sun along straight-edges at the equinoxes, has found that "the distance of the sun from the surface of the earth, at 40° N., is one million miles (p. 49)." This result is about as near the mark as could be expected from the method employed.

Naturalistic Photography. By P. H. Emerson, B.A., M.B. (London: Sampson Low, Marston, Searle, and Rivington, 1890.)

The quick call for a second edition of this work indicates the approval with which it has been received, and we may safely say there is not a better or more instructive book on the art principles of photography than the one before us. Dr. Emerson is a photographer of the first rank, his artistic compositions are everywhere admired, and the energetic manner with which many of the old and cherished ideas of the ordinary photographer are attacked and others established makes it very manifest that he only writes what he knows to be true. The literary style of the book is excellent, and the exposition has the merit of being strikingly original; it should, therefore, be studied by every photographer, both amateur and professional, who desires to excel in his art.

LETTERS TO THE EDITOR.

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Acquired Characters and Congenital Variation.

Beyond this letter I cannot pursue my interpolated adversary, Mr. Dyer.

The syllogisms which he attributes to me are entirely his own. I willingly admit, therefore, that they are as ingeniously bad as they can well be.

I will now state shortly what my position was, and is:—

(1) The assumed antithesis between "acquired characters" and "congenital variation" has arisen out of the cult of Darwin as opposed to Lamarck.

(2) The theory of Lamarck fails, in my opinion, as much as the theory of Darwin, to give any adequate or satisfying explanation either of the genesis, or of the development, of organic forms.

(3) But the theory of Lamarck is more philosophical than the theory of Darwin, in so far as it seeks for, and specifies, a definite natural cause for the phenomena of variation.

(4) The theory of Darwin is essentially unphilosophical in so far as it ascribes these phenomena to pure accident, or fortuity.

(5) That Darwin himself, at one time, if not always, admitted this idea of fortuity to be a mere provisional resort under the difficulties of ignorance.

(6) That the later worshippers of Darwin depart, in this respect, from their master, and making the weakest part of his system the special object of their worship, have set up Fortuity as their idol.

(7) That it is under the influence of this superstition that they now seek to deny altogether that acquired characters can become congenital.

(8) That this denial is against the most familiar experience of Nature, and especially of artificial selection, which is the antetype and foundation of the whole theory of evolution.

(9) That in all domestic animals, and especially in dogs, we have constant proof that many acquired characters may become congenital.

(10) That it is no answer to this argument to demand proof that the babies of a blacksmith are ever born with the abnormal arm-muscle of their papa.

(11) That in order to avoid and evade the force of innumerable facts proving that many acquired characters may, and do, become hereditary, fortuitists have invented a new verbal definition of what they mean by "acquired."

(12) That this definition is full of ambiguities and assumptions, concealed under plausible words, but the object of which is to limit the meaning of "acquired characters" to gross, visible, palpable changes affecting single individuals, and which the analogies of Nature do not lead us to expect or to suppose can be repeated in a single generation, even if a tendency to their development is really implanted in the race.

(13) That, still farther to render impossible the proof they demand, our fortuitists affix to their definition of the word "acquired," conditions which beg the whole question in dispute. Not only must the new characters be gross, palpable, visible—cases of "hypertrophy," of "extension," or of "thickening,"—but also they must be "obviously due to the direct physical action of the environment on the body of the individual." This is a condition which is irrational. It excludes

all those fine, invisible "molecular" changes, through which Nature habitually works, and it ascribes to mere outward and mechanical agencies, effects which, alone, we have no reason to suppose they ever can produce.

On the question of "prophetic germs," Mr. Dyer challenged me to produce a single case of organs useless now, but in course of preparation for future use. I replied by referring him to this phenomenon as universal throughout Nature in the life-history of every individual organism; and I also referred him to the well-known idea of Darwinian embryology which establishes a close analogy between the laws governing the development of the embryo, and the whole past development of organic life.

Mr. Dyer replies that I ought to have explained this sooner—when challenged to do so by Prof. Ray Lankester—an observation which has nothing to do with the merits of the question. The truth is, I wished to close my dispute with that distinguished Professor, as I now desire to close it with Mr. Dyer, and I was satisfied with an indirect admission that, as regards every individual organism, my assertion could not be contradicted. What this involves, I left, and now leave again, as unexhausted as it is indeed inexhaustible.

In conclusion, I must observe upon the use Mr. Dyer makes of the phrase "*a priori* argument," which he apparently uses not only for all deductive argument, but for all analytical reasoning. When he says he "has not an *a priori* mind," he really means that he is indisposed to all analysis. This is a very common attitude even with many able and distinguished men—especially when they are devoted to a system, and are the disciples of some prophet, whose words and phrases they gulp and swallow whole. It is an attitude which has its use; but it is not one to boast of. Mr. Dyer's declaration that "the questions at issue with regard to evolution are now, I believe, thoroughly understood by biologists" is the most astonishing utterance I have ever heard or read coming from a scientific man. Discussion with him is useless. He and his friends know all about it. How life began, and how it grew from more to more—the whole secret of creation—"an open scroll, before them lies." I am happy to think that I am not the only searcher—by many thousands—whose pens Mr. Dyer must intervene to stop. There is a great army of us who are conscious above all things of the ignorance of man. ARGYLL.

Kinellan, Murrayfield, N.B.

IN the number for January 16 (p. 247) Mr. Thiselton Dyer observes that "there are many readers of NATURE who, while taking a general interest in the problems raised by Darwinism, have not followed all that has been written about it." For the benefit of such persons he gives an interesting explanation of Darwin's views on several important points.

I have not read *all* that has been written, but all, I think, that has ever appeared in the pages of NATURE, and with the result that I am more and more convinced of the inadequacy of the Darwinian theory to account for the origin of species. Natural selection is a *vera causa*, but of very limited operation. The theory of sexual selection but partly removes one serious difficulty not of the first magnitude.

I find Darwinians—not Darwin—very ready to insinuate or assert that an unwillingness to adopt their views, on the part of persons who believe in a supernatural revelation, arises from theological prejudice, which hinders them from listening to the voice of reason. I think there is some prejudice on both sides. For myself, fully believing in a Supreme Designer, I am perfectly and most fearlessly willing that "the attempt at mechanical explanation" should be carried as far as possible, well knowing that "a final universal cause" cannot possibly be disproved or reasonably denied. And Darwinism is committed to no such denial.

We have our choice between two alternatives. Life on our globe had a beginning; and its cause was certainly not mechanical or natural,—for reasons not theological, but strictly scientific, in the technical sense of the word. For, as the laws of Nature operate uniformly, if life had ever commenced spontaneously, it must of natural necessity do so again and again, since it would be most absurd to suppose that only during some previous state of the earth's surface did matter exist in such a condition as to be capable of conversion into living things. If life had ever arisen mechanically, it would require a miracle to *prevent* repetitions of the process.

We have, then, to take our choice between supposing with Darwinians that the life-producing power acted once for all, and supposing that it has acted repeatedly and continuously, in more ways than one. I see no theological, and, let me say, no Scriptural, objection to either. Let it be believed willingly, if good reasons can be given, that all life began with a single germ which could not only produce its like—which is wonderful enough—but which even contained in itself such amazing potentialities that it could become, and has become, the parent of every form of life, sentient or non-sentient, that has ever appeared on our globe.

To me this seems scientifically improbable. For why should the power, whether acting intelligently, or, if anyone prefers it, without intelligence, create one germ only? Why not millions? And if of one kind, why not of many? And if single organisms, why not organisms connected with one another, even in highly complex structures? And why act once only? Why not start non-sentient life at one time, sentient at another? For do not sentient things need a separate germ? I take leave to think so. But be this as it may, they are as much in advance of the non-sentient, however much alike those germs we know of may appear to be, as the non-sentient are of inanimate matter.

The other alternative supposition is that the life-producing power, instead of acting once only, and then subsiding into its primæval torpor, continues to act. That, as it once acted upon inanimate matter, not robbing it of anything, but rather, while availing itself of its properties, conferring upon it new powers, so it has acted since upon living things, ever producing out of the old new and higher forms of life; availing itself of all existing faculties of living things, but while allowing them to achieve all that they can, still moulding fresh forms, and conferring higher faculties. To suppose this, is only to suppose that the action of the life-producing power, since life began, has been analogous to what we know was its action in producing life. It is hardly to be supposed that the production of one marvellous germ has exhausted all its energy.

Yet, if the Darwinian theory can enable us to dispense with the aid of this power, let it do so. Let reason prevail.

Darwinians offer, as an adequate explanation of the formation of new species from the older, that this development comes about simply through natural selection—through the survival of the fittest of favourable variations.

"The origin of any species," says Mr. Thiselton Dyer, "lies firstly in the occurrence, and secondly in the selection and preservation, of a particular variation." But surely a particular variation alone—that is, such as can be brought about, as we know from experience, in a single generation—does not sufficiently differentiate one species from another. Short-horned cattle, for instance, are not a new species, nor would they deserve to be so termed if it should eventually happen that all other varieties of horned cattle became extinct. In the great majority of cases, at all events, there must be *more than one* particular variation, before we can recognize a specific difference. Species have become what they are by the *combination*, in one organism, of many particular variations, each well suited to the rest. No particular variation could make of another ruminant a giraffe. What we want, and what seems to be wanting in the Darwinian theory, is a satisfactory hypothesis to explain the *concurrence* of many particular variations, by the co-existence of which in one structure the new species is constituted. Variations, or "fluctuations," as Mr. Thiselton Dyer has happily termed them, will not account for this. Between some species there may be merely slight and single differences; but Nature can show us much more than this. We often find a complicated apparatus formed by the concurrence in one individual of many particulars of structure combining to produce an effect wholly peculiar.

Take the following instance, or rather group of instances. There are venomous serpents, of many species and in many lands, which differ most widely from the non-venomous kinds, from which, or from the ancestors of which, they are generally believed to have been derived. In these we find, to begin with, teeth which have undergone strange modifications. They are needle-like in shape. They are not fixed in the jaw. They occupy a very prominent position. They have minute perforations, terminating near, but not precisely at, the point. They have muscles by which they may be recurved, so that their points may be directed towards the throat. They have hollows in which to lie. They have muscles by which, on occasions, they may be projected beyond the mouth. Besides all this poison-secreting glands, and poison-bags, and channels of com-

munication with the perforations in the teeth. Further still, a special instinct leading the snake to make use of this wonderful weapon of offence, and suitable nerves to regulate its complicated action.

Now, unless all these numerous variations—and they might fairly be multiplied by subdivision—had in the first instance appeared simultaneously in one individual, and unless all had been duly connected, the whole apparatus would have been useless, and there would have been nothing of which natural selection could avail itself. Useful intermediate forms there can be none. A rifle is a more formidable weapon than a lance or dart, but of what use would be a thing half-way between the two? The venom-discharging apparatus has in it no part which could possibly be dispensed with.

To give one more instance. The tongue of the woodpecker is moved forwards in a singular way; not simply, as usual, by a muscle and sinew in front of the base of the tongue, but by a sinew terminating in a loop, through which passes another sinew from behind the tongue which, doubling through the loop, is attached to the base of the tongue. By this means, when the muscle is contracted, the tongue is drawn forward with a double velocity, which is to this bird specially useful. Now, it is impossible for any ingenuity to devise an action intermediate between this and the usual simple pull in respect of utility or complexity. But there is much more here than "a particular variation." The first woodpecker that possessed this structure must have had it in complete order, for otherwise the tongue would not move at all. In that woodpecker it must have commenced to exist in a rudimentary form before birth, in a germ possessing novel powers.

And here I must ask, How is it that anyone questions the Duke of Argyll's statement that "all organs do actually pass through rudimentary stages in which actual use is impossible"? Is it not precisely this which is implied in the Darwinian statement that "from the variable constitution of the ovum probably arises the varying structure of the organism developed from it"? What was afterwards developed was at first rudimentary, and useless. This is equally true of the whole organism—say of the serpent, or of the bird—and of the entirely novel and complicated apparatus found in them.

To call the apparatus in either serpent or bird "a particular variation" would be to give up the whole case for Darwinism. A wonderful *combination of many* particular variations has to be accounted for; and, so far as I can see, Darwinism utterly fails to account for it. There are thousands of cases presenting the same difficulty.

There are simpler cases of specific change, in which the concurrence, the simultaneous appearance, of many slight and particular variations is not indispensable, but only their succession in due order in the course of many generations. Here, there is some room for the theory. Thus perhaps, possibly, we might get a giraffe. But I prefer a theory which, if true at all, accounts as readily for the most complicated apparatus as for the simplest forms of living things. R. COURTENAY.

Hotel Faraglioni, Capri, January 31.

PROBABLY many readers of the recent discussion on the transmission of acquired characters will regret that a more definite conclusion has not been arrived at. This is probably due to the fact that the premises now in our possession do not admit of a definite answer yet being given. Those who assume that there is no evidence in favour of the transmission of acquired characters are mostly, I presume, supporters of "the continuity of the germ-plasm" theory of Weismann. Almost everyone admits that individuals may and do acquire certain characters due to change in environment, use, disuse, &c.; but while many maintain that these characters are transmitted to offspring, others deny that such is the case, or think that the evidence is insufficient. In supporting "the continuity of the germ-plasm" theory it is impossible to suppose that the germ-plasm is continued from one generation to another like a portion of entailed property. For each individual gives off thousands of ova or spermatozoa as the case may be, only a very few of which go to produce new individuals; therefore there is a dissipation of "germ-plasm,"—that is to say, in the germinal cells of mammals of to-day there cannot be any of the identical "germ-plasm" which existed in their remote invertebrate ancestors ages ago. For all this dissipation there must be some constructive process, otherwise the germ-plasm would come to an end.

From whence is derived this constructive material? Clearly from the exterior, for a fertilized ovum obtains material from without to admit of growth and elaboration. The constructive material, then, which the "germ-plasm" obtains—to admit of its liberal dissemination each generation—is derived from the external world, *viâ* the organism with which it is incorporated, or indeed of which it forms a part. Seeing, then, that the organism—from which this germinal matter is derived—can acquire characters—that is, undergo certain definite changes in response to altered conditions—then it seems reasonable to suppose that that part of it which ultimately finds its way to the germ-cells, is also modified during its transmission, and will therefore have more or less effect upon the forthcoming generation. But how much variation is due to the above cause, and how much to the almost infinitely various possible combinations of the two unlike germinal elements, it is impossible to say.

J. COWPER.

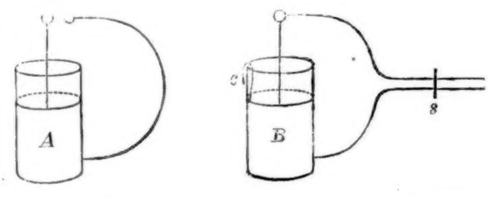
Easy Lecture Experiment in Electric Resonance.

AN experiment, exhibited by me in its early stages at the Royal Institution a year ago, and since shown here in various forms, on the overflow of one Leyden jar by the impulses accumulated from a similar jar discharging in its neighbourhood, is so simple an illustration of electric resonance, and so easily repeated by anyone, that I write to describe it.

Two similar Leyden jars are joined up to similar fairly large loops of wire, one of the circuits having a spark-gap with knobs included, the other being completely metallic, but of an adjustable length.

The jar of this latter circuit has also a strip of tinfoil pasted over its lip so as to provide an overflow path complete with the exception of an air-chink, *c*. It is important that this overflow path be practically devoid of self-induction. A jar already perforated could be well utilized for the purpose.

Then if the two circuits face each other at a reasonable distance, and if the slider, *s*, is properly adjusted, every discharge of A causes B to overflow. A slight shift of the slider puts them out of tune.



Instead of thus adjusting by variable self-induction, my assistant, Mr. Robinson, has made a slight modification by using a condenser of variable capacity, consisting of two glass tubes coated with tinfoil, one sliding into the other, and joined by a flexible loop of wire; an easy overflow from one coat to the other being likewise provided. On making this loop face the discharging circuit of an ordinary Voss machine with customary small jars *in situ*, bright sparks at the overflow gap occur whenever the common machine sparks are taken, provided the sliding condenser be adjusted to the right capacity by trial.

There is little or no advantage in using long primary sparks; the vibrations are steadier and more definite with short ones. It is needless to point out that the 2 jars constitute respectively a Hertz oscillator and receiver, but fair precision of timing is more needed with these large capacities than with mere spheres or discs, because the radiation lasts longer and there are more impulses to accumulate. Hence actual resonance as distinguished from the effect of a violent solitary wave is better marked. Moreover, the sparks are bright enough to be easily seen by a large audience. OLIVER J. LODGE.

University College, Liverpool.

African Monkeys in the West Indies.

WITH reference to the note in NATURE of February 13 (p. 349), on the occurrence of an Old-World monkey in Barbados, I may point out that the same West African monkey (*Cercopithecus callitrichus*) has also been introduced and is now found wild in St. Kitts (cf. Sclater, P.Z.S., 1866, p. 79). It likewise

occurs in Nevis, whence the Zoological Society received living specimens (presented by Mr. Graham Briggs) in 1870.

The only West Indian island in which Quadrumana of the *American* type occurs is Trinidad, which was, doubtless, formerly part of the mainland of South America.

3 Hanover Square, W., February 17. P. L. SCLATER.

Galls.

I HAD not intended to take any further part in this correspondence; but the interesting suggestion which has now been made upon the subject by Mr. T. D. A. Cockerell (NATURE, Feb. 13, p. 344) induces me to withdraw the sentences that he quotes from my previous letters, to the effect that it seems impossible to imagine any way in which galls can be attributed to natural selection acting on the plants *directly*. In my own consideration of the matter this seemed "obvious," and therefore my motive in taking up the difficulty as presented by Mr. Mivart was that of "asking whether anybody else had a better explanation to offer" than the one which my letter suggested—viz. "that natural selection may operate on the plants *indirectly through the insects*," by always selecting those insects the character of whose secretions is such as will best cause the plants to grow the particular kind of morphological abnormality which the larvæ require. Mr. Cockerell, however, has now furnished what seems to me an extremely plausible hypothesis, showing that there is a way in which it is quite conceivable that the growth of galls may be an actual benefit to the plants, and therefore that natural selection may act directly on the plants themselves in evolving these sometimes highly specialized structures for the use of their parasites. Mr. Cockerell informs me in a private communication that he has been verifying this hypothesis by observations in detail; but whether or not he will be able to establish it, I think at any rate he has done good service in thus suggesting another possibility.

On the other hand, I cannot see that Mr. Ainslie Hollis has helped us at all (NATURE, January 23, p. 272). For he merely enunciates the truism that trees which were not endowed with sufficient "developmental vigour" adequately to resist the attacks of gall-making insects "would doubtless have long ago succumbed in a struggle for existence." And this truism he appears to suppose furnishes an explanation of how "natural selection, operating in the ordinary manner," has produced galls for the exclusive benefit of the insects. But it is obvious that the more detrimental the growth of galls has proved to trees, the less reason there must have been for natural selection, "operating in the ordinary manner," to have developed these often highly specialized structures for the benefit of parasites.

London, February 13. GEORGE J. ROMANES.

The Supposed Earthquakes at Chelmsford on January 7.

NATURE for January 16 (p. 256) reprints from the *Essex County Chronicle* a short account of two supposed earthquake-shocks felt at and near Chelmsford on January 7, at 12.30 and 1.25 p.m. Being engaged in the study of British earthquakes, I made inquiries in the district referred to, and the result of these is to show that the shocks were almost certainly due to the firing of unusually heavy guns at Woolwich. It may be worth while to state the evidence for this conclusion somewhat fully, as it will be difficult to obtain it in after years.

(1) I applied to the authorities at Woolwich and Shoeburyness as to the nature of the firing on January 7. At the latter place, the only practice was from 9-inch and 10-inch guns, the maximum charge used was 70 pounds of powder, and therefore not capable of producing the shocks felt at Chelmsford. At Woolwich, however, the 110-ton gun, "*the heaviest in H.M. service*," was fired at the times mentioned.

(2) *Form of the Disturbed Area.*—The only accounts I have as yet received are from the following places: Great Warley (near Romford), Brentwood, Epping, Ingatestone, on the road between Ongar and Fyfield, Roxwell, Chelmsford, Chignall St. James, and Chipping Hill (Witham); which are respectively at about 6, 12½, 14, 16, 16, 21, 24, 24, and 32 miles distance from Woolwich. Referring to a map of Essex, it will be seen that these places all lie close to a line drawn from Woolwich in a north-easterly direction; with the exception of Epping, the direction of which is about north by east from Woolwich. According to the *Times* weather report of January 8, southerly and south-westerly breezes prevailed very generally throughout the kingdom on the previous day.

(3) *Nature of the Shock.*—In four cases, the shock was in the first instance attributed to the firing of heavy guns. If there was any vibration of the earth, it must have been very slight, and the following descriptions seem to leave little doubt that the rattling of windows noticed was due to an air-wave.

Great Warley—The shock "broke a pane of glass 4 feet × 2 feet on my job."

Brentwood—"The shocks commenced as a low rumble, increasing till the doors shook and rattled, as though the rumbling was followed by a bang or explosion."

Between Ongar and Fyfield (the observer driving)—"The ground felt as if it were sinking," and there was "a rumbling noise something like guns in the distance."

Roxwell—The sound "exactly resembled the report of the big guns at Shoebury, but was far louder than we usually hear them."

Chelmsford (the observer walking)—There was "a noise as of a very heavy weight being rolled across the floor of the room of the house to the south of him, which he was passing."

Chignall St. James—"The shock was extremely slight, but there was a most pronounced concussion in the air which made a sound on the windows as if a person had thumped the centre of the window frame with the soft part of his hand. There was no tremulous motion felt."

Witham—The observer "heard a strange rumbling sound which seemed to slightly deafen him, but he felt no vibration of the earth."

That the disturbances recorded had only one origin is, I think, evident, (1) from the decrease in intensity (roughly speaking) as the distance from Woolwich increases, and (2) from there being no considerable gap between the places of observation. Records from the immediate neighbourhood of Woolwich could hardly be expected, as there they would naturally be attributed to their proper source.

I am indebted to the editor of the *Essex County Chronicle* for inserting a letter asking for observations on the shocks, and to several gentlemen for the courtesy and kindness with which they replied to this letter and to other inquiries that I made in the surrounding district. CHARLES DAVISON.

38 Charlotte Road, Birmingham, February 13.

Shining Night-Clouds.

IN July last, on a fine night, about 8 p.m. (two hours after sunset), I noticed a fleecy cloud lit up by a yellowish light, directly over the back of a range of hills due west from this place. As it did not move, it struck my attention, and I observed that what little wind there was carried the few floating clouds north-east to south-west. I continued to watch the cloud, which covered say 4° or 5°, until 11 p.m., and concluded that as in that direction lay the Puracé volcano, about 40 miles away, the light and cloud probably came from it. But I made inquiries by telegraph, and found that no eruption had taken place in the Puracé, which has been quiet now for many years. I regret, seeing now that the subject is interesting, that I did not observe more carefully. I may add that in the direction of the cloud no prairie or forest fire could have occurred to account for it. ROBERT B. WHITE.

Agrado (lat. 2° 20′ N.), Department of Tolima,
U.S. of Colombia, S.A., December 22, 1889.

A Greenish Meteor.

TO-NIGHT (Jan. 30), at 8.15 p.m., I saw a meteor which, notwithstanding a bright moon, shone out exceedingly brightly, exceeding any star. It appeared to travel south, for about 10°, vanishing about 15° above the horizon. Its colour differed from that of any meteor I have seen before, being pale green or greenish. T. D. A. COCKERELL.

West Cliff, Custer Co., Colorado, January 30.

THE MOLECULAR STABILITY OF METALS, PARTICULARLY OF IRON AND STEEL.

(1) ALLOW me to add some words relative to the very timely lecture on the hardening and tempering of steel, recently published by Prof. Roberts-Austen

(NATURE, xli. pp. 11, 42). I desire, in the first place, to point out the bearing of the singular minimum of the viscosity of hot iron (*loc. cit.*, p. 34) on the interpretation given of Maxwell's theory of viscosity (*Phil. Mag.* (5), xxvi. pp. 183, 397, 1888 ; xxvii. p. 155, 1889). When iron passes through Barrett's temperature of recalescence, its molecular condition is for an instant almost chaotic. This has now been abundantly proved (cf. John Hopkinson, Phil. Trans., London, clxxx. p. 443, 1889, where the literature may be found ; cf. Osmond, below). The number of unstable configurations, or, more clearly, the number of configurations made unstable because they are built up of disintegrating molecules, is therefore at a maximum. It follows that the viscosity of the metal must pass through a minimum. Physically considered, the case is entirely analogous to that of a glass-hard steel rod suddenly exposed to 300°. If all the molecules passed from Osmond's β state to his α state together, the iron or steel would necessarily be liquid. This extreme possibility is, however, at variance with the well-known principles of chemical kinetics. The ratio of stable to unstable configurations cannot at any instant be zero. Hence the minimum viscosity in question, however relatively low, may yet be large in value as compared with the liquid state.

(2) My second point has reference to the function of carbon in steel. It is not to be understood that we ignore the importance of the changes of carburation produced by tempering steel. To explain the varied physical phenomena which accompany temper, it is sufficient to recognize some *special* instability in the tempered metal. This is given by the carbide configuration, and the physical explanations in question may be made without specifying its nature further. Hence the permissibility of the purely physical considerations.

On the other hand, it is indeed surprising that, on the part of engineers and chemists, the important subject of temper has been but inadequately dealt with, as Prof. Austen justly remarks. Sir Frederick Bramwell (cf. NATURE, xxxviii. p. 440), in his inaugural address at Bath, in 1888, dwelt at some length on the subject of temper. The question is again touched upon by Mr. Anderson at the Newcastle meeting of the British Association. Neither of these gentlemen, however, really shows forth the gist of the matter. Indeed, even in Ostwald's massive "Lehrbuch der Allgemeinen Chemie" (Leipzig, W. Engelmann, 1887), full of examples as it is, bearing on all points of chemical physics, the frequent and exceptionally important case of tempered steel is altogether absent. And yet the chemical interpretation to be given to the phenomena of temper seems to be closely at hand. Dr. Strouhal and I (*Wied. Ann*, xi. p. 390, 1880 ; Bulletin U.S. Geol. Survey, No. 14, chap. ii., 1885) showed that, by the process of hardening, the electrical resistance of steel may be increased by more than three times its value for the soft metal. If the hard rod is now softened, the resistance again decreases by an amount depending on the temperature to which the hard metal is exposed and on the time of such exposure, in a way which, throughout the whole research, is beautifully sharp and characteristic. Eventually, the relatively low resistance of soft steel is again reached. Now suppose the carbon molecule of steel to be dissolved in the metal, forming an alloy of Matthiessen's Class II. Seeing that the quantity of carbon contained is not large, the electrical resistance of hard steel is at once an expression of its chemical composition, structurally unknown though it be. Hence in the electrical diagram of the phenomena of temper constructed by Dr. Strouhal and myself, the time variations of resistance of hard steel at any given temperature may be interpreted as a case of Wilhelmy's (*Pogg. Ann.*, lxxxi., pp. 413, 499, 1850) rate of chemical reaction (*Reactionsgeschwindigkeit*), and expressed in accordance with his well-known exponential law. This indeed is the character of the observed time curves. Hence also the full diagram of the phenomena of temper, considered both in their variation with time and with temperature, is available for the elucidation of most points relative to the effect of temperature on rate of chemical reaction.[1]

(3) A further remark may be made relative to Osmond's (*Annales des Mines*, July–August, 1888, pp. 6–7 ; *Mém. de l'Artillerie de la Marine*, Paris, 1888, p. 4) iron of the α and the β type. The assertion that mere strain partly changes α into β iron is in conformity with the viscous behaviour of the metal. For it appears that the effect of any mechanical strain as well as of temper, is marked decrease of the viscosity of the metal. Osmond's theory, however, appears to explain too much. Since most metals can be similarly hardened by straining, it would follow that there should be α and β varieties in all these cases, even though a molecular change corresponding to Gore's phenomenon in iron has only in a few instances been observed (iron, nickel, platinum-iridium alloy). I believe, however, that there is reason to be urged even in favour of this extreme view.[2] The ion theory of metallic conductivity is fast gaining ground.

J. J. Thomson states it in his well-known book ("Applications of Dynamics," p. 296): Giese (*Wied. Ann.*, xxxvii. p. 576, 1889) has outlined an ion theory of electric conduction, uniformly applicable to metals, electrolytes, and gases. It seems to me, if a preliminary hypothesis be made relative to the evolution of a magnetic field out of an electric field ; if advantage be taken of the spiral distribution of points which frequently results from the symmetrical interpenetration of two congruent Bravais systems ;[3] if, finally, in metals, the function performed by a bodily transfer of ions can also be performed by an exchange of the charges of charged atoms (Giese, indirectly Helmholtz), that the possibility of an ion theory of magnetism may be suspected. Quite apart from the influence of a field, the conditions of exceptionally close approach favourable to the transfer of charges from atom to atom, are given by the distribution of the heat agitation in the metal.

(4) I will close this note by some remarks on the change of the character of diffusion when occurring in solids. Studying the coloured oxide coats on iron, Dr. Strouhal and I (Bull. U.S.G.S., No. 27, p. 51, 1886) pointed out that the outer surface of the film is oxidized as highly as possible in air ; and that the inner surface of the film, continually in contact with iron, is reduced as far as possible. This distribution of the degree of oxidation along the normal to the layer, is equivalent to a force in virtue of which oxide is moved through the layer, from its external surface to its internal surface. The formation of an oxide coat is thus a case of diffusion. Conformably with this view, the film, during its formation, behaves like an electrolyte, as was pointed out by Franz, Gaugain, and Jenkin, and more recently by Bidwell and by S. P. Thompson.

We then adverted to the crucial difference between diffusion in solids and diffusion in liquids, inasmuch as in the former case (solids) diffusion demonstrably *ceases* after a certain small thickness is permeated. The limit thickness of the film is reached asymptotically, through infinite time. It has a definite value for each temperature, increasing as temperature increases. In the light of other evidence since gained, this explanation is substantiated. The formation of the

[1] An ulterior consideration presents itself here relative to an extension of the theory of Arrhenius (*Wied. Ann.*, iv. p. 391, 1878) to metallic conductivity. Arrhenius and Ostwald find in the maximum of electrolytic conductivity a measure of rate of reaction. I must pass over this question here, since it is without immediate bearing on the text.

[2] I have spent much time in endeavouring to throw light on this question, and will indicate the results later. My methods were (1) to find the effect of mechanical strain on the carburation of steel ; (2) to find the effect of strain on the rate of solution ; (3) to find the hydro-electric effect of stretching.

[3] A good account of the relations of the Bravais and the Sohncke system is given by H. A. Miers, in NATURE, xxxix. p. 277.

oxide coat is a case of solid diffusion, and as such it bears the same relation to the diffusion of liquids, that the viscosity of solids bears to the viscosity of liquids. The two phases (solid, liquid) of each phenomenon are to be correlated in ways essentially alike. The available stress, as compared with the available instability at a given temperature, determines the time character of the result. CARL BARUS.

Physical Laboratory, U.S. Geological Survey, Washington, D.C.

CHRISTOFORUS HENRICUS DIEDERICUS BUYS BALLOT.

BUYS BALLOT was born on October 10, 1817, at Kloetinge in Zealand; was a student in arts and the natural sciences at the University of Utrecht, where he first became Lector of Physics and Chemistry in 1844, and then successively Professor of Mathematics in 1847, and of Experimental Physics in 1870, which latter chair he ceased to hold in November 1887 on completing his fortieth year as Professor. He was appointed Director of the Royal Meteorological Institute of the Netherlands in 1854, and held this position with great ability and distinction till his death on Monday, the 3rd of the present month.

His first contribution to science was a paper on a chemical subject in 1842, this being a science of which he was Lector at the time; but soon thereafter he turned his attention to meteorology, which he emphatically made the business of his life. The following are among the earlier of his papers on the subject, and they are, it will be seen, very significant of his future work:—"On the Influence of the Rotation of the Sun on the Temperature of our Atmosphere," in 1846; "On the Importance in Meteorology of Deviations from the Mean States of the Atmosphere," in 1850; "Results of the Observations of 1849 and 1850 in different places in Holland," in 1851; and "On Synchronous Representations of Weather Phenomena," in 1854.

In these early times of meteorology, when instruments and modes of observing still greatly needed the guiding hand of science towards the founding of international meteorology, Dr. Buys Ballot was wisely led to attempt the construction of no general isobaric and isothermal maps in investigating storms and other weather phenomena, but contented himself in investigating weather disturbances by representing them over the surface of Europe by means of deviations from the means, or averages, of the places represented. In this mode of working he made several of his more important contributions to meteorology, and out of it developed the system of storm warnings he issued for Holland. In this connection his barometric and thermometric means for a very large number of places over Europe will long be a standard work. Of these contributions, unquestionably the most important is that known as BUYS BALLOT'S LAW OF THE WINDS, which states the relation between the direction of the wind and the distribution of atmospheric pressure at the time the wind is blowing. This relation was further developed by Dr. Buchan in 1869, in his paper on the mean pressure of the atmosphere and prevailing winds of the globe, in which it was shown that the prevailing winds of all climates are simply the result of the distribution of pressure.

One of the most exhaustive discussions of the influence of the moon on weather was made by Dr. Ballot. The discussion covered a period of about a century, and he showed that the longer the period the closer do the cases for or against any such influence approach equality. Subsequent to Maury, Dr. Ballot was one of the earlier and most energetic and successful workers in maritime meteorology, and his meteorological charts of the routes of Dutch ships over the great oceans is a standard work. Dr. Ballot also took an active and efficient part in the Meteorological Conferences and Congresses held at intervals from 1872 to 1888, which have brought about a greater uniformity in meteorological observations and discussions than previously existed. He was chosen, by the first Congress, President of the Permanent Committee. Among his last works was the proposal of a method of developing and representing the variability of the weather and climates by the values of the deviations of the daily observations from the averages, irrespective of sign.

The great merits of his indefatigable services to science, but more particularly to meteorology, were recognized by his being made LL.D. of Edinburgh University, Knight of the Order of the Netherland Lion, Commander of the Order of Franz Joseph of Austria, and of St. James of the Sword of Portugal, and Knight of second class of the Prussian Order of the Crown. But above all, his ever readiness in every degree to oblige, the genial sunshine of his face, and his lovableness, make his death to be felt by many of us as a sharp personal bereavement.

NOTES.

ON Tuesday evening the Cambridge University Natural Science Club and the Master of Downing (Dr. Alex. Hill) gave a *conversazione* at Downing Lodge, at which 260 guests, including many distinguished residents and non-residents, were present. The several scientific professors were very liberal in lending the treasures from their museums, and as this is the first entertainment of the kind which has been given in Cambridge, many objects of great historic interest, such as Babbage's calculating machine, Cavendish's apparatus, &c., were exhibited. Artificial silk was spun, quartz filaments drawn, smokeless gunpowder and other scientific novelties shown. One of the most interesting exhibits was a series of Egyptian heads unwrapped from their mummy cloths, and artfully "restored" by Prof. Macalister. A very attractive feature of the entertainment was an address by Dr. Lauder Brunton, who had much that was interesting to say about his recent experiences in India. Mr. Gardiner illustrated the dispersion of seeds by the aid of the limelight and boxes of seeds of various kinds suspended from the ceiling.

THE annual general meeting of the Geological Society of London will be held to-morrow (Friday) at 3 o'clock, and the Fellows and their friends will dine together at the Criterion Restaurant at 7.30 p.m.

BEFORE the next ordinary meeting of the Royal Microscopical Society, it will have moved its quarters from the rooms hitherto occcupied by it in King's College, which are now required for the purposes of the College, to 20 Hanover Square. The ordinary meetings will in future be held on the third instead of the second Wednesday in the month, and the annual meeting in January instead of February. The Quekett Microscopical Club has also transferred its place of meeting to 20 Hanover Square since the commencement of the year.

WE regret to have to record the death of Sir Robert Kane, F.R.S. He died after a short illness on Sunday, the 16th inst., at his residence in Dublin.

THE fine buildings of the University of Toronto were almost wholly destroyed by fire last Friday. The flames were unfortunately fanned by a strong wind, and the fire spread so rapidly that hardly anything could be saved. A small number of specimens in the museum, and some of the scientific apparatus, were brought out by students, but they were mostly broken while

being removed. The Canadians are justly proud of the University of Toronto, and will no doubt provide for it even more splendid buildings than those which are now in ruins.

SIGNOR SELLA'S views of the Caucasus have been on exhibition in the Royal Geographical Society's map-room since Friday last, and will continue to be exhibited till the close of the month.

WE print elsewhere Prof. David P. Todd's record of work done by the U.S. Scientific Expedition to West Africa, 1889, of which he was director. This is one of several bulletins printed on board the U.S.S. *Pensacola*.

IN the engineering notes from North-West India, of *Engineering* of the 14th inst., we find a most interesting account of the testing of the Chenab Bridge, near Mooltan. This bridge consists partly of seventeen spans of 200 feet, which are of mild steel throughout. These trusses are of the Whipple-Murphy type, with raking heel posts; the ties are at an angle of 45°, and consequently the depth is a tenth of the span. In previous girders of this type, made in iron, the deflection under full loads was usually less than 0·0004 of the span, while here $1\frac{1}{2}$ inch, equal to 0·0006, obtains throughout, and in each case the observed permanent set is less than $\frac{1}{8}$ inch in the whole thirty-four girders in the viaduct. *Engineering* observes that "there is thus no question of bad workmanship either in the pieces sent out from home or in the erection at site, and it is very clear that steel structures, especially when so light as these spans, which only weigh, with corrugated floor and all bearing and expansion gear, 220 tons each, are necessarily more sensitive than those of iron."

THE new number of the *Internationales Archiv für Ethnographie* (Band ii. Heft vi.) opens with a valuable paper, by Prof. G. Schlegel, of Leyden, on Siamese and Chinese-Siamese coins. This contribution is illustrated by a coloured plate. Of the other papers, the most important is an account of the Nanga of the Fiji Islands, by Mr. Adolph B. Joske, Fiji. These remarkable stone inclosures, now ruined, were first brought to the notice of anthropologists by the Rev. Lorimer Fison, of the Australasian Wesleyan Mission. Three of them have been visited by Mr. Joske, and he is thus enabled to give the plan of an inclosure drawn from his own measurements. His paper has been edited by Baron Anatole von Hügel, who adds instructive notes. In another paper, Prof. Giglioli gives an interesting account of a remarkable stone axe and stone chisel in use among the Chamacocos of South-East Bolivia.

WE are glad to observe that in the Ceylon estimates for the current year provision is made for an increased vote of Rs. 10,000 for archæological purposes. Sir Arthur Gordon, in explaining the vote, said, "It is proposed to make some systematic examination of the interesting remains at Sigiri, and to commence on a modest scale, before the rapidly disappearing monuments of the past have altogether perished, a species of archæological survey resembling that carried on in India. Such an examination should be completed in about three years, and the vote is proposed to cover the salary and travelling expenses, for 1890, of the officer selected for the purpose."

A LARGE and rich collection of specimens of amber, illustrating all the varieties found in the amber district of North Germany, has lately been sent to the New York School of Mines by one of its earliest graduates, Mr. H. A. Demelli, now a resident of Berlin. At a recent meeting of the New York Academy of Sciences, this collection was examined with great interest by the members, and Dr. Newberry, the President, read an instructive paper on amber. After the reading of the paper, Dr. N. L. Britton spoke of the occasional occurrence of amber in New Jersey, in connection with the lignites so abundant in the Cretaceous and Eocene beds; and Mr. George F. Kunz exhibited several specimens of American amber, one of which—from Mexico—excited much admiration. Mr. Kunz said that during the last fifteen or twenty years travellers had occasionally brought specimens of a very remarkable amber from some locality in Southern Mexico. The only thing known about this amber is that it is taken to the coast by natives, who report that it occurs in the interior so plentifully, and in such large pieces, that they use it for making fires. It is of a rich, deep golden yellow, and, when viewed in different positions, it exhibits a remarkably green fluorescence, like that of certain petroleums. It is perfectly transparent, and, according to Mr. Kunz, even more beautiful than the famous so-called opalescent or green amber found at Catania, Sicily.

A FRESH illustration of the way in which foreign plants may become "weeds" under new and favourable conditions is afforded by *Melilotus alba* in the Western States of America. It was introduced a few years ago as a garden-plant, and has spread so rapidly in the rich bottom-lands along the Missouri River that, according to *Garden and Forest*, it is fast driving out the sunflower and other native weeds. It is commonly called "Bokhara clover."

AT the meeting of the Scientific Committee of the Royal Horticultural Society, on February 11, Dr. Oliver and Prof. Scott presented an interim report on the investigations undertaken by them respecting the effects of London fogs on plants under glass. Specimens of orchids affected by fog had been received from Messrs. Veitch and Son, Chelsea; and of tomato plants from the superintendent of the Royal Horticultural Society's gardens at Chiswick. On the suggestion of the chairman, it was decided that the chemical constituents of London fog should be investigated, and that the exciting causes of the injury to plants should be traced. In order that the work might be carried out under advantageous circumstances, it was resolved that application should be made to the Government Grant Committee of the Royal Society for pecuniary aid.

AT the same meeting of the Royal Horticultural Society's Scientific Committee, Mr. McLachlan drew attention to a disease in sugar-cane at St. Vincent, where in some localities about 25 per cent. of the crop would be lost this year. According to Mr. Herbert Smith, who had examined the canes, a beetle of the family Scolytidæ, and the larva of a moth, were concerned. It is probable that the beetles enter the canes only by the exit holes of the moths, and that the moth is a widely spread species, already known to attack sugar-cane in other countries.

IN the January number of the *American Naturalist* Mr. R. E. C. Stearns begins what promises to be an interesting series of papers on the effects of musical sounds on animals. His first paper deals with "dogs and music." From his friend, Prof. George Davidson, of California, he has received the following instance:—"A small black-and-tan named 'Bessie,' belonging to Mr. A. B. Corson, of North Fifth Street, Philadelphia, will, on hearing 'Shall we meet beyond the river?' sung, throw her head back and set up a most dismal howl, while the tears will run down her cheeks. If the tune is played solemnly on an organ and no word spoken, the same thing will occur; but if any of the words are spoken, with not the slightest musical intonation, she will run to the speaker, and beg and plead in her own way, and do everything but speak, to have it stopped."

THE *Annalen der Hydrographie und Maritimen Meteorologie* for December, published by the German Admiralty, contains an interesting discussion by Dr. W. J. van Bebber, on the dependence of the force of the winds upon the surface over which they blow. It is generally admitted that the winds at sea are, under

similar circumstances, stronger than on land; but actual comparisons, such as the author has undertaken, are not frequently made. He has chosen two stations on the coast—viz. Cherbourg and Hurst Castle—having a different position with regard to the sea, but at which the observations are made under nearly similar conditions. The results of careful comparisons under eight points of the compass, for a period of several years, plainly show that in all months the northerly and north-easterly winds at Cherbourg are considerably stronger than at Hurst Castle, and that the southerly winds at Cherbourg fall considerably short in strength of those at Hurst Castle. The tables show that the strong winds coming from the sea are on an average one degree of Beaufort's scale (1–12) heavier than those coming from the land, while, with lighter or local winds, the difference often amounts to two degrees of the above scale. Information of this kind should be of use to fishermen and others when putting to sea.

M. PLANTAMOUR gives, in a recent number of the *Archives des Sciences*, the results of his eleventh year's observations of periodic movements of the ground, as shown by spirit-levels. It appears that, while in general the east side sinks with lowering of temperature, and rises with a rise, these movements do not always follow with the same rapidity. A sudden change of temperature produces at once a rise or sinking of the east side; but the maxima of the ground-positions rarely coincide with the maximum or minimum of temperature. This eleventh year is exceptional in that the extremes of temperature are but one or two days in advance of those of the movements, whereas in previous years the retardation has been a fortnight to four months behind minimum temperature, and a fortnight to three months behind maximum. In two years (1881 and 1885) the maximum of rise was even four days before the maximum of temperature. Thus, while temperature seems to be the chief cause of the oscillations, some other opposing cause must be at work. M. Plantamour compared the eleven years' mean effects with the variations in solar intensity, but failed to detect any relation.

CARL HESS, the German naturalist, has proved by minute microscopical investigation that the eye of the mole is perfectly capable of seeing, and that it is not short-sighted, as another naturalist (Kadyi) would have us believe. Hess maintains that, in spite of its minute dimensions,—1 millimetre by 0·9 millimetre—the eye of this little creature possesses all the necessary properties for seeing that the most highly-developed eye does; that it is, indeed, as well suited for seeing as the eye of any other mammal, and that in the matter of refraction it does not differ from the normal eye. In order to bear out the theory of short-sightedness, the physiological reason was adduced that in its subterranean runs the mole is accustomed to see things at close distances, and that its eye had become gradually suited to near objects. But to this Hess objects that the mole when under ground most probably makes no use of his eyes at all, as it would be impossible to see anything owing to the absence of light, but that when he comes to the surface, and especially when he is swimming, he does use his eyes. In order to accomplish this, he only has to alter the erect position of the hairs which surround and cover his eyes, and which prevent the entry of dirt when he is under ground, and at the same time to protrude his eyes forward.

IT seems rather strange that, while skins and eggs of the Great Auk are so highly valued, the public rarely hear of Pallas's Cormorant, the extinction of which in the North Pacific corresponds to that of the Great Auk in the North Atlantic. Only four specimens of Pallas's Cormorant are known to exist in museums; no one possesses its eggs; and no bones were found or preserved until Mr. Leonhard Stejneger, of the Smithsonian Institution, was so fortunate some years ago as to rescue a few of them. Yet this bird was the largest and handsomest of its tribe. So says Mr. Stejneger in an interesting paper—just issued by the Smithsonian Institution—in which he records how the bones referred to were found by him in 1882 near the north-western extremity of Behring Island. In an appendix to this paper Mr. Stejneger's "find" is fully and exactly described by Mr. Frederic A. Lucas.

WE have received the first two numbers of the *Scottish Journal of Natural History*. This monthly periodical is intended to be mainly a chronicle of the work done by the different Natural History Societies in Scotland; but short papers on subjects connected with Natural History will also be given, and we notice that articles have been promised by well known men of science, including Profs. James Geikie, G. J. Romanes, and many others. At present very few of the Scottish Natural History Societies print Transactions; so there is ample room for the new venture, and we wish it all success. Communications are to be addressed to the Editors, care of the publisher, Mr. W. B. Robinson, 194 Sauchiehall Street, and 105 New City Road, Glasgow.

THE first part of the Memoirs and Proceedings of the Manchester Literary and Philosophical Society for the current session has been issued. It contains a paper by Mr. Charles Bailey, on the discovery near Ribblehead of *Arenaria gothica*, a plant new to Britain, the typical form of which has so far been recorded only for two Swedish localities. The Ribblehead specimens are stated to be more robust than those from Sweden. The issue also includes a paper by Mr. Charles H. Lees on the law of cooling and its bearing on the theory of heat in bars; and the first part of Mr. Faraday's "Selections from the (unpublished) Correspondence of Colonel John Leigh Philips, of Mayfield, Manchester" (1761–1814). The latter includes letters from Dr. Henry Clarke (the mathematician), James Sowerby, and a number of other persons of local eminence during the latter half of the last century.

PROF. WEISMANN requests us to state that in his article on Heredity, printed in NATURE on February 6, the sentence beginning on p. 319, line 38, should have read—"Sir William Thomson, in endeavouring to make clear the dispersion of rays of light by conceiving of a molecule as consisting of hollow spheres enclosed one within the other and in contact with one another through springs, never believed," &c.

TWO gaseous fluorides of carbon, the tetrafluoride, CF_4, and the difluoride, C_2F_4, have been isolated, and form the subject of two simultaneous papers contributed to the current number of the *Comptes rendus*. One of these communications is from M. Moissan, whose energy in this domain of chemistry appears untiring. Unlike chlorine, fluorine directly attacks carbon with varying degrees of energy, according to the form in which the carbon is presented. When a current of pure fluorine is passed over the purest form of lamp-black, which has previously been freed from hydrocarbons by digestion with petroleum and boiling alcohol, combination occurs with such energy that the whole of the finely divided carbon becomes instantly incandescent. The lighter varieties of wood charcoal also take fire spontaneously in fluorine, the gas appearing to be first condensed for a few moments, and then the mass becomes suddenly incandescent and throws off brilliant scintillations. If the density of the charcoal is greater, and there is no loose dust upon its surface, it is necessary to warm it to 50°–100° C. in order to bring about combination and its accompanying incandescence. When once the incandescence is started at any spot it rapidly extends throughout the entire mass. Ferruginous graphite requires to be heated to a temperature just below dull redness, and gas retort carbon to full redness, in order to effect combination, while the diamond may be heated for any length of time over a

Bunsen lamp without any alteration in weight being noticeable. The products of combination are generally gaseous mixtures of CF_4 and probably C_2F_4. When the most readily attacked varieties of carbon are employed, and only in small quantities so as to avoid excess, the gas is almost pure CF_4. Carbon tetrafluoride is a colourless gas, which liquefies under a pressure of five atmospheres at 10° C. It is completely absorbed and decomposed by an alcoholic solution of potash with production of potassium fluoride and carbonate. On decomposing the latter salt with an acid the volume of carbon dioxide liberated is the same as that of the carbon tetrafluoride used. CF_4 is slightly soluble in water, more readily in carbon tetrachloride, alcohol, or benzene. Determinations of its density gave numbers which agreed with the formula CF_4. If excess of carbon is heated to redness in a platinum tube, and fluorine allowed to slowly stream through, another gas is obtained on collecting over water which is not capable of being absorbed by alcoholic potash. This gas liquefies at 10° under a pressure of 19–20 atmospheres. M. Moissan does not seem to have yet determined its composition, but it appears likely to be the C_2F_4 described in the second communication by M. Chabrié. M. Moissan also states that CF_4 may likewise be prepared by passing vapour of carbon tetrachloride over silver fluoride heated to a temperature of 300° C. in a glass or metal tube. M. Chabrié shows that both CF_4 and C_2F_4 may be obtained by heating the corresponding chlorides of carbon with silver fluoride in a sealed tube to 220° C. In an actual experiment 5·1 grams of AgF were heated with 1·55 grams of CCl_4 for two hours, at the end of which time the tube, which itself was but little attacked, was opened, and an almost theoretical yield of CF_4 obtained; the gas was totally absorbed by alcoholic potash in accordance with the equation $CF_4 + 6KOH = K_2CO_3 + 4KF + 3H_2O$. When C_2Cl_4 was used instead of CCl_4, a gas whose density corresponded to the formula C_2F_4 was obtained. The experimental density was 3·43; the calculated value for C_2F_4 is 3·46. The spectra of the two fluorides, according to M. Moissan, exhibit the lines of fluorine very clearly, together with several broad bands, resembling the flutings of carbon.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on February 20 = 8h. 3m. 7s.

Name.	Mag.	Colour.	R A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1565	—	—	7 36 25	- 14 29
(2) 27 Cancri	6	Yellowish-red.	8 20 39	+13 1
(3) β Cancri	4	Yellow.	8 10 36	+ 9 32
(4) ζ Canis Min. ...	5	White.	7 46 0	+12 3
(5) 26 Pickering ...	Var.	Reddish-yellow.	7 57 2	- 12 47
(6) S Cygni	Var.	Reddish.	20 3 14	+57 40

Remarks.

(1) "Planetery nebula; pretty bright, pretty small; extremely little elongated." The spectrum has not yet been recorded.

(2) A star of Group II. Dunér states that the bands are very wide and dark in the red, but weaker in the green and blue. He does not, however, state what bands are present. Observations similar to those already suggested for other stars of the group are required.

(3) This is stated to have a fine spectrum of the solar type by Vogel. The usual differential observations are required.

(4) A star of Group IV. (Vogel). Usual observations required.

(5) This star has a very feeble spectrum of the Group VI. type, which has not yet been fully described.

(6) Although Cygnus is not now in the most convenient position for observations, it may still be observed soon after sunset. The variable, S Cygni, has not yet had its spectrum recorded, and the approaching maximum (February 28) may therefore be taken advantage of. Gore states the period as 323 days, and the range as from 8·8–10·1 at maximum to <13 at minimum. If it has a banded spectrum, as may be expected from the colour, the type of spectrum will probably not be difficult to determine, notwithstanding the faintness of the star.

A. Fowler.

Progress of Astronomy in 1886.—An account of the progress of astronomy in the year 1886, by Prof. Winlock, has been issued from the Smithsonian Institution. Although the record is primarily intended to serve as a series of notes for those who have not access to a large astronomical library, the bibliography will be found useful to the professional astronomer as a reference list of technical papers. A considerable amount of useful information is given in this extract from the Smithsonian Report for 1886–87, the section devoted to reports of Observatories being very complete. A subject-index to the review has been effected by inserting the necessary page references to the bibliography.

The Maximum Light-Intensity of the Solar Spectrum.—We have received from Dr. Mengarini his paper on the above subject (*Untersuchungen zur Naturlehre des Menschen und der Thiere*, xiv. Band, 2 Heft). After reviewing the previous work that has been done on the varying intensity of different parts of the spectrum, the author describes the three methods he used in his researches. The observations led him to conclude that the maximum of light-intensity is subject to variability in position from day to day and hour to hour, just as the maxima of thermal and chemical effects of the spectrum, although the sky be clear and the atmosphere steady. Using a prismatic spectrum, it was found that the maximum light-intensity fluctuated between about λ 564 and D, and, generally speaking, was more pronounced in the morning than in the afternoon. Some observations made at Rome in July 1881, on clear or slightly clouded days, showed that the maximum shifted from λ 564·1 to 584·3.

Spectrum of Borelly's Comet, *g* 1889.—Mr. Backhouse, in a letter to the *Observatory*, notes that he observed the spectrum of this comet with a Browning miniature spectroscope on the 15th and 19th ultimo. The three CO bands were very vividly seen, but no other line; on the former date there was a very faint continuous spectrum, but on the latter only a suspicion of such.

Spectra of δ and μ Centauri.—Prof. Pickering, in a communication to *Astronomische Nachrichten*, No. 2951, records that an examination of the photographs of stellar spectra taken by Mr. S. J. Baily at the Harvard Observatory station, near Closica, Peru, shows that the F line due to hydrogen is bright in the spectra of the stars δ and μ Centauri.

On the Star System ξ Scorpii.—Some elaborate researches into the orbits of the components of this system were given by Dr. Schorr in an inaugural dissertation at Munich University last year. All available measures of position-angle and distance have been brought together and compared with those derivable from the new elements found, making the computation of great value.

GEOGRAPHICAL NOTES.

On Tuesday, Dr. Nansen lectured in Christiania on his plan for a North Pole Expedition. He advocates the employment of a ship built with a special view to strength, having its sides constructed at such an angle that, instead of being crushed by the ice, the vessel would be raised by it. The Expedition, he thinks, should advance through the Behring Straits, where the vessel would be carried northward by a favourable current. At the New Siberian Island the vessel would enter the ice-floes. It would then "proceed towards the North Pole, in which direction the current would probably carry it."

The *Colonies and India* gives the last news from Cooktown relating to Sir William Macgregor's explorations in New Guinea. His project was to ascend the Fly River on another voyage of discovery. It seems that Sir William and his party, in a steam launch, dropped anchor in the river on December 14. The

launch stranded, and fifteen canoes, carrying about 150 natives, bore down upon the explorers and commenced a savage attack. The Governor's party opened fire, and the natives promptly beat a retreat. After about half an hour, however, they returned, bringing a pig as a peace offering. Sir William consequently went 180 miles further up the river, and on his return visited the same people again, to find them quite peaceably inclined. The Governor started again on December 26 to explore higher up the Fly River.

THE Survey Department of Burmah has in preparation a new map containing all the latest information derived from the parties sent out by the Department. A preliminary issue omitting all the mountain ranges has recently been published.

SIGNOR G. B. SACCHIERO, Italian Consul at Rangoon, sends to the *Bollettino* of the Italian Geographical Society for December an interesting notice of the savage Chin tribes who occupy the hilly region in the north of Burma about the head-waters of the Irawady. The collective tribal name is variously written Chin, Kyen, Kiyin, Kachin, Kakyen, &c.; but they call themselves Sihù, and according to Signor Sacchiero they evidently belong to the Burmese branch of the Mongol stock. In the districts brought under British rule many have already adopted the Burmese dress, and these can with difficulty be distinguished from the Burmese themselves. But the language is more allied to that of the widespread Karen race, and the Karen alphabet composed by the American missionaries in Lower Burma is well suited for expressing the sounds of the Chin idiom. The Chins themselves have no knowledge of letters; nor have they made any progress beyond the rudest state of social culture. They still go nearly naked, and the women on arriving at the age of puberty are tattooed all over the face with a black pigment, being thus disfigured for life, either to prevent the Burmese or the neighbouring tribes from kidnapping them, or else to distinguish them from the women captured by the Chins from the surrounding peoples. They marry early, the bride requiring the consent, not of her parents, but of an elder brother, and the husband promising not to beat her too much, nor to cut her hair if she behaves well. The family yields obedience to the father alone, who recognizes no authority except that of the village chief, this authority passing in both cases to the youngest son. The men always carry firearms, and make their own gunpowder, using instead of sulphur a seed called *aunglak*, first roasted, and then pounded up with charcoal and saltpetre, three parts of the two first to twenty of the last, and mixing the whole with alcohol, or tobacco juice. Both sexes smoke little Indian hookahs, and their favourite drink is *khaung*, a kind of beer extracted from fermented rice. They live mainly by the chase, and when a boar, stag, or other big game is captured, there are great rejoicings in the village. The quarry is covered from neck to tail in a red cloth, and presented to the "temple," or abode of the *nat* (spirit); then the "friend of the *nat*" (priest) pronounces a blessing on the successful hunter, after which all join in the feast, with much tamtaming, shouting, drinking, and dancing through the village. When they descend to the plains, the Chins are Buddhists, but in their villages spirit-worshippers. Not only every village and every district, but every person has his special *nat*, mostly a malevolent being who requires to be pacified by propitiatory offerings. The vendetta is a universal institution, feuds being inherited from family to family, from tribe to tribe, and thus leading to constant bloodshed. If a man is drowned, his son reeks vengeance on the water where he perished by piercing it with spears or slashing it about with long knives. Many of the Chins have already tendered their submission to the British authorities, and arrangements are now in progress for extending orderly government over the whole territory.

ON SOME NEEDLESS DIFFICULTIES IN THE STUDY OF NATURAL HISTORY.[1]

A LITTLE while ago I read, in the preface to a work on natural history, that the book was "of little value to the scientific reader, but that its various anecdotes, and its minute detail of observation would be found useful and entertaining."

What, then, may the "scientific reader" be expected to desire? He must be, in my opinion, a most unreasonable man, if he does not thankfully welcome anecdotes of the creatures he wishes to study, when these anecdotes are the result of patient and accurate observation. For it is precisely such information, that is conspicuously absent from many scientific memoirs and monographs; the author generally spending his main space and strength in examining the shape and structure of his animals, and in comparing one with another, but giving the most meagre details of their lives and habits.

Which, then, is the more scientific treatment of a group of animals—that which catalogues, classifies, measures, weighs, counts, and dissects, or that which simply observes and relates? Or, to put it in another way, which is the better thing to do—to treat the animal as a dead specimen, or as a living one?

Merely to state the question is to answer it. It is the living animal that is so intensely interesting, and the main use of the indexing, classifying, measuring, and counting is to enable us to recognize it when alive, and to help us to understand its perplexing actions.

But, it may be objected, that because the study of the living animal is the more interesting, it is not necessarily the more scientific; indeed, that the amount of entertainment, which we may get out of the pursuit of natural history, has nothing to do with the question at all; that by science we mean accurate knowledge presented in the most suitable form; that shape, structure, number, weight, comparison are the fundamental notions, with which sciences of every kind have to deal; and that scientific natural history is more properly that which takes cognizance of a creature's size, form, bodily organs, and relation to other creatures, than that which concerns itself with the animal's disposition and habits.

I can fancy that I already hear some of my audience say: "But why set up any antagonism between these two ways of studying a creature? Both are necessary to its thorough comprehension, and our text-books should contain information of both kinds; we should be told how an animal is made, where it ought to be placed among others of the same group, and also how it lives, and what are its ways."

Precisely; that is just what memoirs and text-books ought to do; but what, too often, they do *not*. We read much of the animal's organs; we see plates showing that its bristles have been counted, and its muscular fibres traced to the last thread; we have the structure of its tissues analyzed to their very elements; we have long discussions on its title to rank with this group or that; and sometimes even disquisitions on the probable form and habits of some extremely remote, but quite hypothetical ancestor—some "archirotator" (to take an instance from my own subject) who is made to degrade in this way, or to advance in that, or who is credited with one organ, or deprived of another, just as the ever-varying necessities of a desperate hypothesis require:—but of the living creature itself, of the way it lives, of the craft with which it secures its prey or outwits its enemies, of the home that it constructs, of its charming confidence or its diabolical temper, of its curious courtship, its droll tricks, its games of play, its fun and spite, of its perplexing stupidity coupled with actions of almost human sagacity—of all this, this which is the real natural history of the animal, we, too often, hear little or nothing. And the reason is obvious, for in many cases the writer has no such information to give; and, even when he has, he is compelled by fashion to give so much space to that which is considered to be the more scientific portion of his subject, that he has scant room for the more interesting. Neither ought we to be surprised if a writer is "gravelled for the lack of matter," when he comes to speak of an animal's life; for the study of the lives of a large majority is a difficult one. It requires not only abundant leisure, but superabundant patience, a residence favourably situated for the pursuit, and an equally favourable condition of things at home. The student, too, must be ready to adopt the inconvenient hours of the creatures that he watches, and be indifferent to the criticisms of those that watch *him*. If his enthusiasm will not carry him, without concern, through dark nights, early mornings, vile weather, fatiguing distances, and caustic chaff, the root of the matter is not in him. Besides, he ought to have a natural aptitude for the pursuit, and know how to look for what he wants to see; or if he does not know, to be able to make a shrewd guess: and, above all, when circumstances are not favourable, to have wit enough to invent some means of making them so. And yet when the place, the man, the animals, and the circumstances all seem to promise a rich harvest of observations, how often it happens that some luckless accident, a snapt twig, a

[1] The Presidential Address to the Royal Microscopical Society, at the annual meeting, on February 12, 1890, by Dr. C. T. Hudson, F.R.S.

lost glass, a hovering kestrel, a sudden gust of wind, a roving dog, or a summer shower, robs the unlucky naturalist of his due; nay, it sometimes happens that, startled by some rare sight, or lost in admiration of it, he himself lets the happy moment slip, and is obliged to be contented with a sketch from memory, when he might have had one from life.

But I have not yet got to the bottom of my budget—the heaviest trouble still remains; and that is, that the result of a day's watching will often go into a few lines, or even into a few words; and so it happens, that the writer of the history of a natural group of animals is too frequently driven to fill up his space with minute analysis of structure, discussions on classification, disputes on the use of obscure organs, or descriptions of trifling varieties; which, exalted to the rank of species, fill his pages with wearisome repetitions; for were he, before he writes his book, to endeavour to make himself acquainted with the habits of all the creatures he describes, his own life-time might be spent in the pursuit.

We will now take a different case, and suppose that many years have been spent in the constant and successful study of the animals themselves; and that the time has come, when the naturalist may write his book, with the hope of treating, with due consideration, the most interesting portion of his subject. He is now beset with a new class of difficulties, and finds that publishers and scientific fashion alike, combine to drive him into the old groove: for the former limit his space, by naturally demurring to a constantly increasing number of plates and an ever lengthening text; while the latter insists so strongly on having a complete record of the structure, and points of difference, of every species, however insignificant, that it is hardly possible to do much more than give that record—a mere dry shuck, emptied of nearly all that makes natural history delightful.

And so we come round again to the point that I have already glanced at, viz. "Ought natural history to be delightful?"

Ought it to be delightful! Say, rather, ought it to exist? What title has the greater part of natural history to any existence but that it charms us? It is true that this study may help—does help many—to worthier conceptions of the unseen, to loftier hopes, to higher praise; that it gives us broader and sounder notions of the possible relation of animals, not only to one another, but also to ourselves; that it provides us with the material for fascinating speculations on the embryology of our passions and mental powers; and that it may even serve to suggest theories of the commencement and end of things, of matter, of life, of mind, and of consciousness—grave questions, scarcely to be dealt with successfully by human faculties, but in a condition to be discussed with infinite relish.

When I speak, then, of the pleasure we derive from the study of natural history, I include these graver and higher pleasures in the word.

Here and there, too, no doubt, the knowledge of the powers and habits of animals is materially useful to us; and, indeed, in the case of some of the minuter organisms, may be of terrible importance; but, in that of the large majority of creatures, we might go out of the world unconscious of their existence (as, indeed, very many people do), and yet, unlike the little jackdaw, not be "a penny the worse." For what is a man the better for studying butterflies, unless he is delighted with their beauty, their structure, and their transformations? Why should he learn anything about wasps and ants, unless their ways give him a thrill of pleasure? What can the living plumes of the rock-zoophytes do for us, but 'witch our eyes with their loveliness, or entrance us with the sight of their tiny fleets of medusa-buds, watery ghostlets, flitting away, laden with the fate of future generations?

When, at dusk, we steal into the woods to hear the nightingale, or watch the night-jar, what more do we hope for than to delight our ears with the notes of the one, or our eyes with the flight of the other? When the microscope dazzles us with the sight of a world, whose inhabitants and their doings surpass the wildest flights of nightmare or fairy tale, do we speculate on what possible service this strange creation may render us? Do we give a thought to the ponderous polysyllables that these mites bear in our upper world, or to their formal marshalling into ranks and companies, which are ever being pulled to pieces, to be again re-arranged? No! it is the living creature itself which chains us to the magic tube. For there we see that the dream of worlds peopled with unimagined forms of life—with sentient beings whose ways are a mystery, and whose thoughts we cannot even guess at—is a reality that lies at our very feet; that the air we breathe, the dust that plagues our nostrils, the water we fear to drink, teem with forms more amazing than any with which our fancy has peopled the distant stars; and that the actions of some of the humblest arouse in us the bewildering suspicion, that, even in these invisible specks, there is a faint foreboding of our own dual nature.

If, then, we make some few exceptions, we are entitled to say that the study of natural history depends for its existence on the pleasure that it gives, and the curiosity that it excites and gratifies: and yet, if this be so, see how cruelly we often treat it. Round its fair domain we try to draw a triple rampart of uncouth words, elaborate, yet ever-changing classifications, and exasperatingly minute subdivisions; and we place these difficulties in the path of those whose advantages are the least, those who have neither the vigorous tastes that enable them to clear such obstacles at a bound, nor the homes whose fortunate position enables them to slip round them. For modern town life forces a constantly increasing number of students to take their natural history from books; and too often these are either expensive volumes beyond their reach, or dismal abridgments, which have shrunk, under examination pressure, till they are little else than a stony compound of the newest classification and the oldest woodcuts.

But the happier country lad wanders among fields and hedges, by moor and river, sea-washed cliff and shore, learning zoology as he learnt his native tongue, not in paradigms and rules, but from Mother Nature's own lips. He knows the birds by their flight, and (still rarer accomplishment) by their cries. He has never heard of the *Œdicnemus crepitans*, the *Charadrius pluvialis*, or the *Squatarola cinerea*, but he can find a plover's nest, and has seen the young brown peewits peering at him from behind their protecting clods. He has watched the cunning flycatcher leaving her obvious, and yet invisible young, in a hole in an old wall, while it carried off the pellets that might have betrayed their presence; and has stood so still to see the male redstart, that a field-mouse has curled itself up on his warm foot and gone to sleep. He gathers the delicate buds of the wild rose, happily ignorant of the forty-odd names under which that luckless plant has been smothered; and if, perchance, his last birthday has been made memorable by the gift of a microscope, before long he will be glorying in the transparent beauties of *Asplanchna*, unaware that he ought to crush his living prize, in order to find out which of some half-dozen equally barbarous names he ought to give it.

The faults, indeed, of scientific names are so glaring, and the subject is altogether so hopeless, that I will not waste either your time or my patience by dilating on it. But, while admitting that distinct creatures must have different names, and very reluctantly admitting that it seems almost impossible to alter the present fashion of giving them, I see no reason why these, as well as the technical names of parts and organs, should not be kept as much as possible in the background, and not suffered to bristle so in every page, that we might almost say with Job, "There are thistles growing instead of wheat, and cockle instead of barley."

We laughed at the droll parody in which the word *change* was defined as "a perichoretical synechy of pamparallagmatic and porroteroporeumatical differentiations and integrations," yet it would not be a difficult matter to point out sentences, in recent works on our favourite pursuits, that would suggest a similar travesty. No doubt, new notions must often be clothed in new language, and the severer studies of embryology and development require a minute precision of statement that leads to the invention of a multitude of new terms. Moreover, the idea that the meaning of these terms should be contained in the names themselves is excellent; but I cannot say that the result is happy—I might almost say that it is repulsive; and if we suffer this language to invade the more popular side of natural history, I fear that we shall only write for one another, and that our scientific treatises will run the risk of being looked at only for their plates, and of being then bound up with the Russian and Hungarian memoirs.

The multiplication of species, too, is a crying evil, and the exasperating alterations of their names, in consequence of changing classifications, is another. The former, of course, is mainly due to the difficulty, no doubt a very great one, of determining what shall be a species, and what a variety. How widely experts may differ on this question, Darwin has shown, by pointing out that, excluding several polymorphic genera and many trifling varieties, nearly two hundred British species, which are generally considered varieties, have all been ranked by

botanists as species; and that one expert has made no fewer than thirty-seven species of one set of forms, which another arranges in three. Besides, even in the cases where successive naturalists have agreed in separating certain forms, and in considering them true species, it happens now and then, as it did to myself, that a chance discovery throws down the barriers, and unites half-a-dozen species into one.

Under these circumstances one would have expected that the tendency would have been to be chary of making new species, and no doubt this is the practice of the more experienced naturalists; but, among the less experienced, there is a bias in the opposite direction; and all of us, I fear, are liable to this bias when we have found something new; for, even if it is somewhat insignificant, we are inclined to say with Touchstone, "A poor thing, sir, but mine own!" Now, were this fault mended, much would be avoided that tends to make monographs both expensive and dull; for, though the needs of science require a minute record of the varieties of form, which are sometimes of high importance from their bearing on scientific theories, yet the description of them, as varieties, may often be dismissed in a line or two, when nothing further is set forth than their points of difference; whereas, if these forms are raised to the rank of species, they are treated with all the spaced-out dignities of titles, lists of synonyms, specific characters, &c., &c., and so take up a great deal of valuable room, weary the student with repetitions, and divert his attention from the typical forms.

But when everything has been done that seems desirable, when names and classification have been made both simple and stable, and the number of species reduced to a minimum, there will still remain the difficulty that monographs must, from the nature of the case, generally be grave, as well as expensive books of reference, rather than pleasant, readable books, within the reach of the majority. I would suggest then, that, if it be possible, each group of animals should be described not only by an all-embracing monograph, to be kept for reference on the shelves of societies like our own, but by a book that would deal only with a moderate number of typical, or very striking forms; that would describe them fully, illustrate them liberally from life, and give an ample account of their lives and habits.

Such a book should give as little of the classification as possible; it should avoid the use of technical terms, and above all, it should be written with the earnest desire of so interesting the reader in the subject, that he should fling it aside, and rush off to find the animals themselves. By this means we should not only get that active army of out-of-door observers, which science so greatly needs; but, by bringing the account of each group into a reasonable compass, we should enable students of natural history to get a fair knowledge of many subjects, and so greatly widen their ideas and multiply their pleasures.

For why should we be content to read only one or two chapters of Nature's book? To be interested in many things—I had almost said in everything—and thus to have unfailing agreeable occupation for our leisure hours, is no bad receipt for happiness. But life is short, and its duties leave scant time for such pursuits; so that to acquire a specialist's knowledge of one subject would often be to exchange the choice things of many subjects for the uninteresting things of one. And how uninteresting many of them are! How is it possible for any human being to take pleasure in being able to distinguish between a dozen similar creatures, that differ from one another in some trifling matter; that have a spike or two more or less on their backs, or a varying number of undulations in the curve of their jaws, or differently set clumps of bristles on their foreheads? Why should we waste our time, and our thoughts, on such matters? The specialist, unfortunately, must know these things, as well as a hundred others equally painful to acquire and to retain, and no doubt he has his reward; but that reward is not the deep delight that is to be found in the varied study of the humbler animals; of those beings "whom we do but see, and as little know their state, or can describe their interests or their destiny, as we can tell of the inhabitants of the sun and moon; . . . creatures who are as much strangers to us, as mysterious, as if they were the fabulous, unearthly beings, more powerful than man, yet his slaves, which Eastern superstitions have invented."

Those, then, who are blest with a love of natural history should never dull their keen appreciation of the wonders and beauties of living things, by studying minute specific differences; or by undertaking the uninteresting office of finding and recording animals, that may indeed be rare, but which differ from those already known in points, whose importance is due solely to arbitrary rules of classification.

This eagerness, to find something new, errs not only in wasting time and thought on matters essentially trivial and dull, but in neglecting things of the greatest interest, which are always and everywhere within reach. Take, for instance, the case of *Melicerta ringens*. What is more common, what more lovely, than this well-known creature? And yet how much there remains to be found out about it. No one, for example, has ever had the patience to watch the animal from its birth to its death; to find out its ordinary length of life, the time that it takes to reach its full growth, the period that elapses between its full growth and death, or, indeed, if there be such a period. And yet even these are points which are well worth the settling. For, if *Melicerta* reaches its full growth any considerable time before the termination of its life, it would seem probable that, owing to the constant action of its cilia, it would either raise its tube far above the level of its head, or else be constantly engaged in the absurd performance of making its pellets and then throwing them away. Who has ever found it in such a condition, or seen it so engaged? yet the uninterrupted action of the pellet cup would turn out the six thousand pellets, which form the largest tube that I am acquainted with, in about eight days, and those of an average tube in less than three; while the animal will live (according to Mr. J. Hood)[1] nearly three months in a zoophyte trough, and no doubt much longer in its natural condition. It is true that the creature's industry in tube-making is not continuous. It is often shut up inside its tube, when all ciliary action ceases; and, moreover, when expanded, it may be seen at times to allow the formed pellet to drift away, instead of depositing it; but, allowing for this, there is no little difficulty in understanding how it is that, with so vigorous a piece of mechanism as the pellet-cup, the tube at all ages, except the earliest, so exactly fits the animal. I am aware that it has been stated that the whole of the cilia (including those of the pellet-cup) are under the animal's control, and that their action can be stopped, or even reversed, at pleasure. But this, I think, is an error. Illusory appearances, like those of a turning cog-wheel, may be produced by viewing the ciliary wreath from certain points, and under certain conditions of illumination; and these apparent motions are often reversed, or even stopped, by a slight alteration either in the position of the animal, in the direction of the light, or in the focussing of the objective. When, however, under any circumstances, the cilia themselves are distinctly seen, they are invariably found to be simply moving up and down; now turning sharply towards their base, and now recovering their erect position. Even the undoubtedly real reversal of the revolution of the pellet in its cup, which is constantly taking place, can be easily explained by purely mechanical considerations, and consistently with the continuous up and down motion of the cilia. Moreover, of the actual stoppage of the cilia, in the expanded Rotiferon, I have never seen a single instance. In all cases, on the slightest opening of the corona, the cilia begin to quiver, and they are always in full action, even before the disk is quite expanded; while, should a portion of the coronal disk chance to be torn away, its cilia will continue to beat for some time after its severance: so that there is good reason for believing, that the ciliary action is beyond the animal's control.

It is possible, indeed, that *Melicerta* may continue to grow (as Mr. Hood says that the Floscules appear to do) as long as it lives; or it may adopt the plan of some species of *Œcistes*, which, to prevent themselves from being hampered by their ever-growing tubes, quit their original station at the bottom of the tube, and attach themselves to it above, creeping gradually upwards as the tube lengthens. At any rate it would be interesting and instructive to watch the growth of a *Melicerta*, and the building of its tube, from the animal's birth to its death. An aquarium, in which *Melicerta* would live healthily and breed freely, could easily be contrived, and a little ingenuity would enable the observer to remove any selected individual to a zoophyte trough and back again, without injury; and his trouble perhaps would be further repaid by such a sight as once delighted my eyes at Clifton, where I picked, from one of the tanks of the Zoological Gardens, some *Vallisneria*, whose ribbon-like leaves were literally furred with the yellow-brown tubes of

[1] Mr. Hood, of Dundee, has kept in his troughs *Melicerta ringens* for 79 days, *Limnias ceratophylli* for 83 days, *Cephalosiphon limnias* for 89 days: the *Floscularia* usually lived about 50 days; but *F. Hoodii* died, before maturity, in 16 days.

Melicerta. I coiled one of these round the wall of a deep cell, and thus brought into the field of view, at once, more than a hundred living *Melicertæ* of all ages and sizes, and all with their wheels in vigorous action; a display never to be forgotten.

Such a tank, so stocked and managed, would probably enable a patient and ingenious observer to decide several other points, about which we are, at present, in ignorance: to say whether the same individual always lays eggs of the same kind, or whether it may lay now female eggs, now male, now ephippial eggs; and to say what determines the kind of egg that is to be laid; whether it is the age of the individual, or the supply of food, or temperature, or sexual intercourse that is the potent cause.

It would, too, hardly be possible for the male, to escape the observation of a naturalist, who possessed a tank in which were hundreds of *Melicertæ*: and the male is as yet almost unknown.

Judge Bedwell found in the tubes of the female, in winter, a small Rotiferon resembling the supposed male, that I had seen playing about *M. tubularia*; only the former had a forked foot, and sharp jaws that were at times protruded beyond the coronal disc. Its frequent occurrence in the tubes in various stages of development, and the nonchalance with which the female suffered it to nibble at her ciliary wreath, inclined the observer to conclude, that the animal was the long sought-for male. Unfortunately it was only observed when in motion, so that its internal structure was not made out; and the matter therefore still rests in some doubt.

No doubt it is a strong argument that the female would probably suffer nothing but a male to take such liberties with her; but it would seem, from the following account, that it is possible for such freedoms to be pushed too far.

Mr. W. Dingwall, of Dundee, was on one occasion watching a male Floscule circling giddily round a female, and constantly annoying her by swimming into her fully expanded coronal cup. Again and again she darted back into her tube, only to find her troublesome wooer blocking up her cup, and sadly interfering with, what to a Floscule is, the very serious business of eating—for these animals will often eat more than their own bulk in a few hours. It was clear at last that the lady would not tolerate this persistent interference with her dinner; for when—after waiting, rather a longer time than usual, closed up in her tube—she once more expanded, only to find him once more in his old position, she lost all patience, and effectually put an end to his absurdities, by giving one monstrous gulp, and swallowing her lover. It will not surprise you to hear that he did not agree with her, and that after a short time she gave up all hope of digesting her mate, and shot him out into the open again, along with the entire contents of her crop. He fell a shapeless, motionless lump; the two score and ten minutes of a male Rotiferon's life cut short to five; but, strange to say, in a second or two, first one or two cilia gave a flicker, then a dozen; then its body began to unwrinkle and to plump up; and, at last, the whole corona gave a gay whirl, and the male shot off as vigorous as ever, but no doubt thoroughly cured of its first attachment.

I have taken *Melicerta ringens*, as an example of what yet remains to be done, even with an animal which is as common in a ditch, as a fly is in a house; but almost every other Rotiferon would have done equally well, for there is scarcely a single species, whose life-history has been thoroughly worked out.

To me, natural history in many of its branches seems to resemble a series of old, rich mines, that have been just scratched at by our remote ancestors, and then deserted. Our predecessors did their best with such feeble apparatus as they had; it was not much, perhaps, but it was wonderful that they did it at all with no better appliances; and it irks me to think that we, who are equipped in a way which they could not even dream of, should turn our backs on the treasures lying at our feet, and go off prospecting in new spots, contented too often with a poor result, merely because it is from a new quarter.

Besides, the love of novelty is a force too valuable to be wasted on a mere hunt for new species in any one group of animals, especially unimportant ones. It should rather be used to make us acquainted with the more striking forms of many groups. Let us have no fear of the reproach of superficial knowledge; everyone's knowledge is superficial about almost everything; and even in the case of those few who have thoroughly mastered some one subject, their knowledge of that must have been superficial for a great portion of their time. Indeed, the taunt is absurd. I can imagine that a superficial knowledge of law, or surgery, or navigation may bring a man into trouble; but what possible harm can it do himself, or anyone else, that he is content with knowing five Rotifera instead of five hundred? And yet if any naturalist were to study only *Floscularia*, *Philodina*, *Copeus*, *Brachionus*, and *Pedalion*, it would give him the greatest possible pleasure, as well as an excellent general notion of the whole class. Let any tyro at the seaside watch the ways and growth of a *Plumularia*, or of a rosy feather-star, his knowledge of the groups to which they belong could certainly not be dignified even with the term "superficial"—"linear" or "punctiform" would be more appropriate; but the pleasure, that he would derive from such a study, could not be gauged by counting the number of animals that he had examined. It would depend on the man himself; and might, I should readily imagine, far exceed that derived by the study of a hundred times the number of forms in books; especially when such a study had been undertaken, not from a natural delight in it, but from some irrelevant reason, such as to support a theory, to criticize an opponent, to earn a distinction, or to pass an examination.

In truth that knowledge of any group of animals, which would rightly be called superficial when contrasted with the knowledge of an expert, is often sufficient to give us a satisfactory acquaintance with the most interesting creatures in it; to make us familiar with processes of growth and reproduction too marvellous to be imagined by the wildest fancy; and to unfold to us the lives of creatures who, while possessing bodily frames so unlike our own that we are sometimes at a loss to explain the functions of their parts, yet startle us by a display of emotions and mental glimmerings, that raise a score of disquieting questions.

Moreover, there is another excellent reason why we should not confine our attention to one subject; and that is, that even the most ardent naturalist must weary at times of his special pursuit. Variety is the very salt of life; we all crave for it, and in natural history, at all events, we can easily gratify the craving. If we are tired of ponds and ditches, there are the rock-pools of our south-western shores, and the surface of our autumn seas. A root of oar-weed torn at random from a rocky ledge, an old whelk shell from deep water, a rough stone from low-water mark, the rubbish of the dredge,—each and all will afford us delightful amusement. It is wonderful, too, what prizes lurk in humble things, and how often these fall to beginners. The very first time that I tried skimming the sea with a muslin net, I picked a piece of green seaweed off the muslin, intending to throw it away; but, seeing a little brown spot on it, I dropped the weed (not a square inch) into a bottle of sea-water, instead. At once the brown speck started off and darted wildly round the bottle. It was too small to be made out with the naked eye, but by the time I had brought my lens to bear, it had vanished. I hunted all over the bottle, and could see nothing, neither with the lens nor without it. I was half inclined to throw away the water; but, as I was certain that I had seen something in it two minutes before, I corked up the bottle and took it home. When I next looked at it, there was the little brown creature flying about as wildly as ever. I soon made out, now, that I had caught a very tiny cephalopod—something like an octopus—and with a pipette I fished it out, and dropped it into a glass cell. At least I dropped the water from the pipette into the cell; but the animal itself had vanished again; I could not see it either in the bottle or the cell. I was not going to be tricked again; so I pushed the cell under the microscope, and there was my prize; motionless, but for its panting; and watching me, as it were, up the microscope with its big blue-green eyes. It was almost colourless, and was dotted at wide intervals with very minute black spots, set quincunx fashion—spots absolutely invisible to the sharpest unaided sight.

As I looked it began to blush—to blush faint orange, then deeper orange, then orange-brown; a patch of colour here, another there, now running across one side of the body, now fading away, again to appear on a tentacle; till at last, as it recovered from its alarm, each black spot began to quiver with rapid expansions and contractions, and then to spread out in ever varying tints, till its wavering outlines had met the expansions of its neighbouring spots; and the little creature, regaining its colour and its courage at the same moment, rushed off once more in a headlong course round the cell.

I was the merest beginner when I saw this, but I had the good luck, knowing nothing whatever about it, and never having given the subject a thought, to see, with my own eyes, how effectually cuttlefishes are protected by their loss of colour, and also to see how the loss takes place.

No doubt the sea-side of our south-western coasts—I mean its creeks, not "the thundering shores of Bude and Bos"—is a paradise for microscopists; but there is no need that we should travel so far afield. Our inland woods, our lanes and pastures, will yield to us a thousand beauties and wonders. The scarlet pimpernel will show its glorious stamens, the flowers of the wound-wort glow like a costly exotic; wild mignonette will rival in its fantastic shape the strangest orchid; the humblest grass will lift a tuft of glistening crystals; the birch and salad-burnet shake out their crimson tassels; the Jungermanns will display their mimic volcanoes, the mosses unfold the delicate lacework of their dainty urns. But the time would fail me to name one tithe of those sources of wonder and delight that lie all around us; and most of which, as in the case of the Rotifera, contain numberless points on which we are all happily ignorant, and therefore in the best of all possible conditions for deriving endless pleasure and instruction from them. Besides, my time and your patience must, I think, be drawing to a close; I would then only once more suggest, that we should not only explore for ourselves all these "pastures new"—no matter how imperfectly—but that we should encourage those, who can be our most efficient guides, to indulge us with the main results in the simplest language. Surely one of the most charming subjects, that can interest human beings, admits of being so treated; and there can be no good reason why the Muse of Natural History (for no doubt there is such a Muse) should resemble that curious nymph among the *Oribatidæ*, whom Mr. Michell found lying under the moss of an old tree, half smothered in a heap of her cast-off skins, admirable types of successive classifications, and abandoned nomenclature.

Happily, however, books in such matters are of little importance; and names and classifications of still less: both these latter, indeed, are of ephemeral interest; they are the pride of to-day, and the reproach of to-morrow. It is to the living animals themselves that we must turn, fascinated not only with their beauty and their actions, but with the questions which the contemplation of them perpetually provokes, and very rarely answers.

For, in the long procession of the humbler creatures, who can tell where life first develops into consciousness, and why it does so; where consciousness first stretches beyond the present so as to include the past, and why that happens; or at what point, and why, memory and consciousness themselves are lighted up by the first faint flashes of reason?

We know nothing now of such matters, and probably we never shall know much; but the mere fact that the study of natural history irresistibly draws us to the consideration of these questions, gives to her pleasant features an undoubted dignity, and raises the charming companion of our leisure hours to the rank of an intimate sharer of some of our gravest thoughts.

THE TOTAL ECLIPSE.

THE U.S.S. *Pensacola* arrived at Saint Paul de Loanda on December 6, after a voyage of 51 days from New York, having made the ports of Horta, Fayal, in the Azores, November 2–3; of Saint Vincent, in the Cape Verdes, November 10–12; of Saint George's Parish, Sierra Leone, November 18–20; and of Elmina, on the Gold Coast, November 26–28.

Immediately on landing at Loanda, it was found that the Rio Quanza steamer, sailing bi-weekly for Muxima, had left two days previously, and that recent washouts along the line of the Caminho de Ferro Trans-Africano made it impracticable for the Expedition to reach either Muxima or Cunga early enough to allow sufficient time for mounting and adjusting the instruments for the eclipse.

I therefore at once decided to locate the Expedition at or near Cape Ledo. Mention should be made here of the courteous civilities of His Excellency the Governor of Loanda, for his kindly interest in the Expedition, and the facilities he offered for the prosecution of the various fields of its work.

The *Pensacola* came to anchor alongside H.M.S. *Bramble* in the little bay to the north of Cape Ledo, on the afternoon of Sunday, December 8. The Eclipse Station was selected in a very favourable spot close to the shore cliffs, and the sites of the principal instruments were determined before night.

A week or ten days' hard work sufficed for getting a large amount of the apparatus in readiness for the eclipse. I placed Prof. Bigelow in charge of the direct photoheliograph of nearly 40 feet focal length, and detailed Mr. Davis to assist him. Mr. Jacoby was intrusted with the charge of the time-determinations, and longitude and latitude work. The *Bramble* was at Cape Ledo on a mission like that of the *Pensacola*, and attending upon the English Eclipse Expedition in charge of Mr. A. Taylor, F.R.A.S.; and through the courtesy of her commanding officer, Captain Langdon, R.N., advantage was taken of her run to St. Paul de Loanda and return, December 14–17, to make a chronometric determination of the longitude, by comparison with the time at Loanda as determined by Mr. Preston, who was left there by the Expedition for the gravity and magnetic work. Also, on the *Bramble's* second return to Loanda, on December 23, another comparison was made.

Prof. Abbe was in charge of the meteorological work and of the organization of parties of observers from the ship's company. A large amount of valuable material results from his work.

The mounting and adjustment of the extensive apparatus for the total eclipse, I reserved for myself. A duplex polar axis eleven feet in length had been constructed of six-inch iron tubings, and mounted with great stability. This axis was driven by powerful clock-work of extreme precision, made by Mr. Saegmueller, of Washington. On this single axis was mounted the totality-battery, consisting of 2 Brashear reflecting telescopes of 8 inches diameter, four Clark telescopes of 3½, 5, 7½, and 8 inches aperture, the second being rigged with an eyepiece enlarging the sun's image to a diameter of 4½ inches, the third being used as a high power directing telescope, while the fourth, a photographic doublet with 10 inch back lens, loaned by the Harvard College Observatory, was arranged for a series of twelve exposures, two of which were made through an orthochromatizing screen provided by Mr. Carbutt; two six-inch Dallmeyer rapid rectilinear lenses of 24 and 38 inches focus; one Schroeder triple objective, of 6 inches aperture and 22 inches focus; one Gundlach orthoscope of 3 inches aperture and 21 inches focus; two flint spectroscopes and one quartz spectroscope loaned by Harvard College Observatory; a duplex photometer of 75 inches focus also provided by Prof. Pickering, and his reversing layer spectroscope for photographing a spectrum trail for fifteen seconds both before and after second and third contacts; a 5 inch Ross lens of 42 inch focus; a 4 inch Spencer objective of 36 inch focus, and a 6·4 inch Merz-Clark objective, both rigged with the means of automatic variation of aperture during totality; and lastly, two duplex cameras provided by Dr. Wright of the Sloane Laboratory of Yale University, for photographic record of the polarization of the corona. In all there were 23 objectives and two mirrors, with their axes adjusted into parallelism.

With the exception of the Gundlach camera, which was reserved for a special investigation of the extreme outer corona, all this apparatus was operated automatically, by an adaptation of the pneumatic organ-valve system of Mr. Merritt Gally, of New York. Exposing shutters were opened and closed, sensitised plates were exchanged for others as soon as exposed, and all the mechanical movements were accomplished with entire precision. Also, by employing an ordinary chronograph in conjunction with the valve system, the exact time of beginning and end of each exposure became a matter of accurate record.

All this apparatus was brought into operation during the period of total eclipse, and over 300 exposures were made in a period of 3m. 10sec.; but no photographs of the corona were secured, as the sun was completely obscured by clouds. However, the entire success of the pneumatic movements is a result of no little value in view of eclipse work in the future.

In addition to this, a silver-on-glass mirror, of 20 inch diameter and 75 feet focal length, by Brashear, lent to the Expedition by Prof. Langley, was so mounted as to throw an image of the corona up the cliff and just underneath the sun at the time of totality. At the focus a beautiful 10 inch image of the sun was formed, and 20 × 24 inch plates of the highest sensitiveness were in readiness to record the coronal streamers. This unusual apparatus was also rendered inoperative by clouds.

With the direct photoheliograph, however, very gratifying success was secured. Seventy pictures of the partial phases were made before totality, and forty after. The serious obstacles to the operation of so long a tube were successfully overcome by means of a skeleton mounting, a combined form of an equatorial stand and tripod; and Prof. Bigelow's sand-clock enabled the precise and easy following of the sun. The revolving plate holder, of 22 inches diameter, actuated automatically by compressed air, in which the principles of the apparatus of the

National Electric Service Company were employed, was a thorough success. Exposures were made at intervals of six seconds.

A few hours before the eclipse came on, the *Pensacola* went out to sea, and stood in the centre of the eclipse-track at the time of totality. Atmospheric conditions were slightly more favourable there than at the main station of the Expedition, and some interesting results were obtained. During totality, however, the clouds were so thick that it is very doubtful whether the true solar corona was seen at all.

The Eclipse Station was completely dismantled by December, 27, and the *Pensacola* left Cape Ledo on the afternoon of the same day.

Returning to Loanda, it was found that two of the three detached parties of the Expedition sent into the interior to observe the eclipse were unsuccessful on account of clouds. The third has not yet been heard from.

DAVID P. TODD.

U.S.S. *Pensacola*, December 31, 1889.

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, December.—Results obtained from Dr. L. Weigert's therapeutic treatment of pulmonary phthisis, by Prof. A. Visconti. Seven patients in various stages of consumption have been subjected to this treatment for the purpose of testing its efficacy. It consists in administering superheated dry air (150° to 180° C.), which is inhaled through a specially prepared apparatus, for which Dr. Weigert claims that it acts directly on Koch's bacillus of tuberculosis. In the incipient stages of the disease satisfactory results were obtained in some respects, such as relief of the cough, greater freedom of respiration, less profuse perspiration, and increased appetite. But it was doubtful whether the germ itself was killed, while in the advanced stages the malady continued its normal development without being perceptibly arrested by the treatment. Without actually condemning Weigert's method, Prof. Visconti cannot at present regard it as an efficacious remedy against phthisis.—On the determination of the coefficient of dynamic and electromotor produce, by P. Guzzi. The author here describes a method of determining this coefficient, for which he claims certain advantages over that proposed by Dr. J. Hopkinson in the *Electrician* of December 3, 1886, especially in the case of engines of over 100 horse-power. His method of calculating the yield of the dynamo and electric motors is based exclusively on electric measurements made with safer and more handy instruments than Hopkinson's dynamometers. Two dynamos of about the same type and dimensions are connected together in such a way that one moves the other as motor, as in the Hopkinson apparatus. But instead of communicating to the system the *dynamic energy* required to maintain it in motion with the velocity and intensity of the normal current, Guzzi's instrument communicates the equivalent *electric energy* derived from any external source whatsoever.

Rivista Scientifico-Industriale, December 31, 1889.—Researches on the absorption of hydrogen by iron, and on the tenacity of certain metals after absorbing gases, by Prof. M. Bellati and S. Lussana. It has already been shown by Hughes (NATURE, vol. xxi., 1880, p. 602) that steel and iron immersed in diluted sulphuric acid become very brittle, and that the same phenomenon is produced when these metals are used as negative electrodes in a voltameter. Prosecuting the same line of research, the authors here describe a series of experiments tending to show that the action of electrolytic oxygen on the tenacity of platinum, and of hydrogen on that of copper and zinc, is uncertain; also, that the absorption of hydrogen produces very probably an increase of tenacity in platinum, as it certainly does in iron, but, on the contrary, a diminution in nickel. Nor can these different results be explained by the simple passage of the current, Möbius having already shown that the elasticity of metals is not perceptibly affected by this cause.—Action of arsenate of hydrogen on potassium permanganate, by D. Tivoli. Some experiments are described, from the results of which the author infers that the solution of potassium permanganate is capable of rapidly and completely absorbing arsenate of hydrogen.—S. Giuseppe Terrenzi gives a somewhat complete list of the land and fresh-water mollusks occurring in the Narni district, Umbria. This fauna presents nothing remarkable, all the species being common to other parts of Umbria, and generally to Central Italy. All are described or mentioned by the Marchese Paolucci in his "Étude de la Faune Malacologique terrestre et fluviale de l'Italie et de ses îles" (Paris, 1878).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 30.—"On the Germination of the Seed of the Castor-oil Plant (*Ricinus communis*)." By J. R. Green, M.A., B.Sc., F.L.S., Professor of Botany to the Pharmaceutical Society of Great Britain. Communicated by Prof. M. Foster, Sec. R.S.

The work embodied in this paper deals (*a*) with the agencies which, during germination, render the reserve materials available for the use of the embryo, (*b*) with the forms in which these are absorbed by it and the mode of their absorption, and (*c*) with the parts played in the process by the endosperm and the embryo respectively.

A ferment is found to exist as a zymogen in the resting seed, which is readily developed by warmth and weak acids into an active condition. The results of its activity are the splitting up of the fat with formation of glycerine and (chiefly) ricinoleic acid. Further changes, brought about by the protoplasm of the endosperm cells, form from the latter a lower carbon acid which, unlike ricinoleic acid, is soluble in water and is crystalline. These changes do not take place in the absence of free oxygen. A quantity of sugar also is formed, which appears to have the glycerine as its antecedent.

The proteids of the seed, which consist of globulin and albumose, are split up by another ferment, with formation of peptone and asparagin.

The only products which enter the embryo are a crystalline acid, sugar, possibly some peptone, and asparagin. Consideration of the structure of the cotyledons, which are the absorbing organs, shows that the mode of absorption is always dialysis.

"Investigations into the Effects of Training Walls in an Estuary like the Mersey." By L. F. Vernon Harcourt, M.A., M.Inst.C.E. Communicated by A. G. Vernon Harcourt, F.R.S.

The present investigations were carried out with a working model of the Mersey estuary, from near Warrington to the open sea beyond the bar. The experiments were directed to the solution of two problems—namely, (1) the influence of training walls in the wide upper estuary on the channel below Liverpool, and across the bar; and (2) the effects of training walls in the lower estuary on the channel across the bar.

The experiments indicate that, whereas training walls in the upper estuary would be injurious, owing to the resulting accretion, training walls in the lower estuary would improve the depth of the outlet channel; and that such training walls, combined with dredging, offer the best prospect of forming a direct, stable, and deepened channel across the bar.

February 6.—"Memoir on the Symmetrical Functions of the Roots of Systems of Equations." By Major P. A. MacMahon, Royal Artillery. Communicated by Prof. Greenhill, F.R.S.

The object of the present memoir is the extension to systems of algebraical quantities of the new theory of symmetric functions which has been developed by the author in regard to a single system in vol. xi. and succeeding volumes of the *American Journal of Mathematics*. In the theory of the single system the conceptions and symbolism are to a large extent arithmetical, and are based upon the properties of single integral numbers and their partitions into single integral parts. In this sense the former theory may be regarded as being unipartite.

In the present generalization to the case of m systems of quantities the fundamental ideas proceed, not from a single number, but from a collection of m single numbers. In regard to number, weight, degree, part, and suffix, the collection of m numbers invariably replaces the single number of the theory of the single system. In this view the theory of the m systems is m-partite.

The quantities, to which the symmetric functions relate may be regarded as the solutions common to m non-homogeneous equations each in m variables. Schläfli, in the Vienna Transactions (*Denkschriften*) for 1852, added another linear non-homogeneous equation in m variables, and then forming the eliminant

of the $m + 1$ equations, thereby obtained an identity which is fundamental in the subject. This identity involves those symmetric functions which are here termed fundamental, and marks the starting-point of the present investigation.

In particular, three distinct laws of symmetry are established, large generalizations of those established by the author in the *American Journal of Mathematics* (vol. xi.). Of these the first two are of fundamental importance, and are examined in detail. A leading idea in these theorems, as in the whole investigation, is the "separation" of a partition; the separation bearing the same relation to the partition as the partition to the number or collection of numbers.

In conclusion, the memoir consolidates and largely generalizes the author's recent researches alluded to above.

February 13.—"On the Unit of Length of a Standard Scale by Sir George Shuckburgh, appertaining to the Royal Society." By General J. T. Walker, R.E., F.R.S.

In the determinations of the length of the seconds pendulum, which were made in London by Kater and at Greenwich by Sabine, and are described in the Philosophical Transactions for 1818, 1829, and 1831, the distance between the upper and lower edges of the pendulum was measured off on a standard scale which had been constructed by Sir George Shuckburgh. The scale had not yet been compared with any of the modern standard scales, but it had been preserved with much care with the instruments appertaining to the Royal Society.

In the autumn of 1888, M. le Commandant Defforges, an officer of the French Geodetic Survey, came to England to take a share in operations for the determination of the difference in longitude between Greenwich and Paris, and also to determine the length of a French seconds pendulum at Greenwich. He kindly undertook to comply with a suggestion which was made to him by me, to compare the portion of Shuckburgh's scale which had been employed by Kater and Sabine with one of the standard metre bars of the International Bureau of Weights and Measures in Paris. The Council of the Royal Society assented, and the scale was sent across to Paris and brought back again by special agent.

The details and results of the comparison are given in a special account by Commandant Defforges, from which it will be seen that the scale was compared with the French metrical brass scale, N, at the temperature of 48°·7 F., at which the distance between Kater and Sabine's divisions, 0 and 39·4, of the Shuckburgh scale was found equal to 1·0006245 metre. On reducing to the temperature of 62° F., which was employed by Kater and Sabine, this distance becomes 1·0007619 metre, which is equivalent to 39·400428 inches if we adopt the relation 1 metre = 39·370432 inches, which was determined by Colonel Clarke, C.B., of the Ordnance Survey, and is given in his valuable work on the comparisons of standards of length. Thus the actual length of the space 0 to 39·4 on the Shuckburgh scale may be regarded with some probability as differing by not more than about 0·0004 inch, or, say, the 100,000th part, from the quantity which the scale indicates.

Physical Society, February 7.—Annual General Meeting.—Prof. Reinold, F.R.S., President, in the chair.—The reports of the Council and of the Treasurer were read and adopted. The former stated that there had been a very satisfactory increase in the number of members during the year. The number now exceeds 360, of whom 80 are Fellows of the Royal Society. During the year the Council had proposed to change the time of meeting of the Society from Saturday afternoon to Friday evening. The change was adopted by the members by a vote of 129 to 30, and had resulted in a larger attendance at the meetings. During the year the second part of vol. i. of the translations of important foreign memoirs had been issued to the members, and it was hoped that a third part would be published early in the present session. The Council had to regret the loss by death of three well-known members—James P. Joule, Warren de la Rue, and Father Perry. A valuable collection of books had been given the Society by the Royal Astronomical Society. From the Treasurer's report, it appeared that the balance of the Society had been increased by £120 during the year. Prof. Hittorf, of Münster, was, at the recommendation of the Council, elected an honorary member of the Society. The result of the new election of officers was declared as follows:—President: Prof. W. E. Ayrton, F.R.S.; Vice-Presidents: Dr. E. Atkinson, Walter Baily, Shelford Bidwell, F.R.S., and Prof. S. P. Thompson; Secretaries: Prof. J. Perry and T. H. Blakesley; Treasurer: Prof. A. W. Rücker, F.R.S.; Demonstrator: C. V. Boys, F.R.S.; other Members of Council: W. H. Coffin, Sir John Conroy, Bart., Conrad W. Cooke, Major-General Festing, F.R.S., Prof. J. V. Jones, Prof. O. Lodge, F.R.S., Prof. W. Ramsay, F.R.S., W. N. Shaw, H. Tomlinson, F.R.S., and G. M. Whipple. Votes of thanks were then passed (1) to the Lords of the Committee of the Council on Education for the use of the room in which the Society met; (2) to the auditors, Prof. Minchin and Dr. Fison; (3) to the President and officers of the Society for their services during the year.—The meeting was then resolved into an ordinary science meeting. Messrs. E. W. Smith and C. E. Holland were elected members of the Society, and Mr. Sidney Evershed was proposed as a member.—The paper on galvanometers, by Prof. W. E. Ayrton, F.R.S., Mr. T. Mather, and Dr. W. E. Sumpner, was then resumed by Prof. Ayrton. A long table of numbers accompanying the paper, and representing the result of experiments on many galvanometers, was explained. From this it appeared that galvanometers of the D'Arsonval type were exceedingly efficient in proportion to the amount of wire used in the coils. It was for this reason that voltmeters with strong permanent magnets could be made sensitive even with an exceedingly large external resistance in series so as to diminish the power absorbed by the instrument. The space occupied by the wire was so exeedingly valuable that the extra resistance did not too much diminish the sensibility. The most sensitive galvanometers should therefore be made of the permanent magnet type. If, however, the magnets were to form part of the moving system, as in most galvanometers, the experiments showed that instruments of the Rayleigh, Gray, or Rosenthal type were the best. The coils should be numerous and small, as Mr. Boys had previously shown. As an astatic system of needles sets itself perpendicular to the earth's field, it was recommended that astatic galvanometers should be placed so that the needles pointed east and west. The controlling magnet would then not need to be turned round as it was raised or lowered. It was recommended to calibrate low-resistance ballistic galvanometers for quantity by measuring the deflection for a known current. This obviates the necessity for large condensers or high potentials. The method, although not new, is not described in text-books. In conclusion, Prof. Ayrton asked for information with regard to microscope galvanometers. C. V. Boys, F.R.S., thought that the factor of merit of galvanometers should not be given in scale divisions per micro-ampere under the condition of constant controlling moment. This gave too great an advantage to instruments of the Gray or Rosenthal type. Great sensibility could be obtained by diminishing the moment of inertia of the suspended parts, the practical limit being determined by the trouble due to the silk fibre. Spider lines, when used in place of silk fibres, gave better results. It was possible by using a good suspending arrangement to use needles $\frac{1}{8}''$ long and a period of 20 seconds, and to gain a sensibility far greater than those indicated in the paper. Ballistic galvanometers should be made with needles as light as possible. The method proposed, of winding the central part of the coil in the opposite sense to the rest, would probably not be good, owing to the unevenness of the field produced. The conclusion come to by the author, that D'Arsonval galvanometers of great sensibility should be made with small coils placed in a very strong field, was one he had himself come to, but had finally abandoned owing to difficulties caused by diamagnetism in the copper and to excessive damping due to Foucault currents. Mr. Swinburne thought that the factor of merit of a galvanometer should be determined differently according as it was to be used for the measurement of current, or quantity, or for null methods merely. He saw no great advantage in making practical instruments proportional. The name D'Arsonval should be dropped, as the instrument denoted by it was invented by Varley years ago. He would like to know the relative sensibility of the telephone and the Lippman galvanometer. Prof. Fitzgerald stated that Lord Rayleigh had shown that the microscope method of observing angular deflections was as sensitive as the ordinary method of mirror and scale, even when only the mirror was used as a pointer, so that if a pointer were attached it would be far more sensitive. The drawback, however, was that it was impossible to distinguish with the microscope between lateral displacements of the needles and the angular motion whose measurement was required. To get over this error it was necessary to read both ends of the pointer, but this was hard to do. Prof. Ayrton replied to the different points raised in the discussion.

Entomological Society, February 5.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The President announced that he had nominated Mr. J. W. Dunning, Captain H. J. Elwes, and Mr. F. D. Godman, F.R.S., Vice-Presidents for the session 1890–91.—Mr. F. D. Godman exhibited a specimen of *Papilio thoas*, from Alamos, Mexico, showing an aberration in the left hind wing. Mr. R. Trimen, F.R.S., remarked that butterflies of the genus *Papilio* were seldom liable to variation.—Mr. C. G. Barrett exhibited a series of specimens of *Phycis subornatella*, Dup., from Pembroke, the east and west of Ireland, the Isle of Man, and Perthshire; and a series of *Phycis adornatella*, Tr., from Box Hill, Folkestone, Norfolk, and Reading; also a number of forms intermediate between the above, taken in the Isle of Portland by Mr. N. M. Richardson. He said that these forms proved the identity of the two supposed species, which he believed were both referable to *P. dilutella*, Hb. He also exhibited specimens of *Hesperia lineola*, and a pale variety of it taken in Cambridgeshire; specimens of *Epischnia bankesiella*, a recently-described species, taken in Portland; and a specimen of *Retinia margarotana*, H.-S., a species new to Britain, discovered amongst a number of *Retinia pinivorana*, which had been collected in Scotland.—Mr. W. H. B. Fletcher showed a series of *Gelechia fumatella*, from sandhills in Hayling Island and near Littlehampton, and, for comparison, a series of *G. distinctella*, from the same places. He also showed a few bred specimens of *G. terrella*, and a series of preserved larvæ. He stated that on the downs the larvæ live in the middle of the tufts of such grasses as *Festuca ovina* and allied species.—Mr. H. Goss read a communication from Dr. Clemow, of Cronstadt, St. Petersburg, on the subject of the coincidence of vast flights and blights of insects during the years 1510, 1757, 1763, 1782, 1783, 1836, and 1847, and the epidemic of influenza. During the year 1889 no unusual activity in the insect world had been recorded. Mr. H. T. Stainton, F.R.S., and Mr. McLachlan, F.R.S., made some remarks on the subject, the purport of which was that there was no connection between epidemics and the occurrence of swarms of insects.—Mr. G. A. J. Rothney communicated a paper entitled "Notes on Flowers avoided by Bees." It appeared, according to the author's observations, made in India, that dahlias were exceptionally attractive, but that the passion-flower was only resorted to by a few species of *Xylocopa*; and that, with one exception, he had never seen any insects feeding on the flowers of the oleander. Mr. Slater, Colonel Swinhoe, Mr. Trimen, Lord Walsingham, and Mr. McLachlan took part in the discussion which ensued.—Dr. D. Sharp read a paper entitled "On the Structure of the Terminal Segment in some male Hemiptera."—Colonel Swinhoe read a paper entitled "On the Moths of Burma," which contained descriptions of several new genera and 107 new species.—Dr. F. A. Dixey read a paper entitled "On the Phylogenetic Significance of the wing-markings in certain genera of the *Nymphalidæ*." A discussion ensued, in which Lord Walsingham, Mr. Jenner-Weir, Captain Elwes, and Mr. Trimen took part.

Zoological Society, February 4.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of January 1890.—A communication was read from Mr. W. K. Parker, F.R.S., containing an account of the morphology of the Hoatzin (*Opisthocomus cristatus*). The author treated of the early stages of the development of this Reptilian Bird, and its shoulder-girdle, sternum, and hind limbs.—A communication was read from Mr. A. D. Bartlett, containing observations on Wolves, Jackals, Dogs, and Foxes. Mr. Bartlett's remarks tended to show that all the varieties of Domestic Dogs owe their origin to Wolves and Jackals, and that the habit of barking has been acquired by, and under the influence of, domestication; also that the Dog is the most perfectly domesticated of all animals.—A communication was read from Mr. G. E. Dobson, F.R.S., containing a synopsis of the genera of the family Soricidæ. The author recognized nine genera, and divided them into two sub-families. New methods of defining the genera were introduced, each genus was briefly characterized, and remarks on certain genera, not admitted in the synopsis (although hitherto generally recognized), were appended.—Mr. F. E. Beddard read a paper containing observations upon some species of Earthworm of the genus *Perichæta*.—A communication was read from Mr. J. M. Leslie, containing notes on the habits and oviposition of the clawed Aglossal Frog (*Xenopus lævis*), as observed at Port Elizabeth, Cape Colony, where this species was said to be of ordinary occurrence.—Mr. Oldfield Thomas read an account of a collection of Mammals from Central Vera Cruz, Mexico, made by a scientific expedition organized by the authorities of the Mexican Museum, under the superintendence of Dr. F. Ferrari-Perez. The collection consisted of about 100 specimens, belonging to 21 species. Amongst these, two (a Hare and a Squirrel) were described as new, and proposed to be called *Sciurus niger melanonotus* and *Lepus veræ-crucis*.

Geological Society, February 5.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—The variolitic rocks of Mont-Genèvre, by Grenville A. J. Cole and J. W. Gregory.—The propylites of the Western Isles of Scotland, and their relations to the andesites and diorites of the district, by Prof. John W. Judd, F.R.S.

EDINBURGH.

Royal Society, January 27.—Rev. Prof. Flint, Vice-President, in the chair.—Prof. Calderwood read a paper on evolution and man's place in Nature. A discussion followed.

February 3.—Sir W. Thomson, President, in the chair.—Dr. William Peddie read a paper on new estimates of molecular distance. He showed that the ratio of the latent heat of vaporisation of a liquid to six times its surface-tension gives an approximation to the number of molecules per linear unit in that liquid. The liquids water, alcohol, ether, chloroform, carbon bisulphide, turpentine, petroleum, and wood spirit, have, according to this method, 50, 52, 30, 15, 19, 30, 40, and 70 millions, respectively, of particles per linear centimetre. Of course no stress is to be laid upon the relative values of these numbers; the point of interest is the complete agreement as to the order of the unknown quantity.—Prof. Tait communicated a paper by Prof. Dittmar on the gravimetric composition of water.—Mr. John Aitken read a paper on the number of dust-particles in the atmosphere of certain places in Great Britain and on the Continent, with remarks on the relation between the amount of dust and meteorological phenomena. He believes that dust condenses moisture before the air is saturated. For the same number of dust-particles per cubic centimetre, the atmospheric transparency depends upon the depression of the wet bulb, being large when the depression is large, but becoming small before the depression vanishes. Increase of temperature also reduces transparency when the number of particles remains the same, for increase of temperature means increase of vapour-pressure. As a rule, quantity of dust decreases when the wind increases. When calms occur dust accumulates. This increases the radiating power of the air, so that it cools quickly and fog forms. Thus a fog may be regarded as a suspended dew.—The dust-measuring instruments intended for use at Ben Nevis were exhibited.

PARIS.

Academy of Sciences, February 10.—M. Hermite in the chair.—Note on an unpublished memoir of Descartes', indicating the right of the author to the priority of a discovery in the theory of polyhedrons, by M. De Jonquières. Some passages are pointed out in the memoir which show that Descartes knew and applied the formula $F+S=A+2$, and furnished the elements of the demonstration, hence his name should be associated with that of Euler as an independent discoverer of the famous formula.—A physical process for the measurement of the inclination of the declination-thread of meridian-circles, by M. Hamy. With ordinary astronomical methods this value can be determined to within half a degree, but using the process described, it is possible to obtain it within a few seconds. The complete description will be given in the coming number (January) of the *Bulletin Astronomique*.—Upon the exponential function, by M. Stieltjes. A demonstration is given of a relation of the form

$$N+e^aN_1+e^bN_2 \ldots\ldots +e^hN_n=0 \quad \ldots\ldots (1)$$

$a, b, \ldots, h$ being whole numbers, $N, N_1, N_2, \ldots N_n$ coefficients. Starting with the polynomial function

$$F(z)=z^\mu(z-a)^{\mu+k_1}(z-b)^{\mu+k_2} \ldots\ldots (z-h)^{\mu+k_n}$$

the author deduces that assuming (1) to hold

$$\int_0^h \phi(z)e^{-z}F(z)dz=0,$$

and then proves this function not to hold if μ be an even number.—Note on a method of transformation in kinematic geometry,

by M. A. Mannheim. In a preceding communication the author has shown how to transform the properties relating to the displacement of a straight line, of which the points describe trajectory surfaces; he now extends his method to the case where the points of the movable line describe trajectory lines only, and taking as examples several theorems relating to the former case, derives therefrom corresponding theorems in the latter.—On a generalization of Euler's theorem relating to polyhedrons, by M. R. Perrin. Attention is drawn to some relations bearing upon Euler's formula, published by the author in 1882 (*Bulletin de la Société Mathématique de France*, t. x.).—On bodies which give a tension of dissociation equal to the tension of the vapour of their saturated solutions, by M. H. Lescœur. Experiments are referred to which are antagonistic to the theory of M. Bakhuis-Roozeboom. According to experiment, the curves representing the tensions referred to as functions of the temperature are tangential, and do not intersect at an acute angle as required by the theory.—Action of fluorine upon different varieties of carbon, by M. Henri Moissan.—A general method for the preparation of fluorides of carbon, by M. C. Chabrié.—On the blue flame of common salt and the spectroscopic reaction of copper chloride, by M. G. Salet. The author finds that the bands seen in the spectrum of salt burning in a common fire, and of which the strongest are situated in the indigo and blue-green, are due to copper chloride, and coincide with bands given by Lecoq de Boisbaudran in his "Spectres Lumineux."—On the electrical resistance of iron and its alloys at high temperatures, by M. H. Le Chatelier. The electrical resistances for a considerable range of temperature of a number of iron alloys have been examined. When the results are graphically shown, the curve for ferro-manganese (13 per cent. Mn) is found to be regular, just as is the case with platinum or platinum-rhodium alloy, while the curves for mild and hard steels show distinctly two singular points at 820° and 710°, and a silicon steel (Si = 3 per cent.) shows the former only. Ferro-nickel (25 per cent. Ni) behaves very peculiarly, as below 550° two modifications having quite distinct properties exist, and nickel itself shows a sudden change of curvature at 340°.—Thermochemical researches upon silk, by M. Léo Vignon. Investigations have been made to determine the heat disengaged when various reagents are absorbed by raw and prepared silk. A discussion of the results seems to indicate that the method may be employed to elucidate the theory of dyeing.—Estimation of potassium and humus in soil, by M. J. Raulin. A method of estimating potassium by weighing it on a tared filter as phosphomolybdate is described, together with the application of the modified permanganate process of J. H. Schmidt to the determination of humus.—On a colouring-matter from Diaptomus, analogous to the carotin of vegetables, by M. Raphael Blanchard. The colouring-matter, isolated from these animal organisms, is shown to differ considerably in spectroscopic properties and in its solubility in alcohol from the lipochromes, and it does not prove to be identical with any of the red pigments from the Cœlenterata, Echinodermata, Bryozoa, or Mollusca; while on the contrary it is found to show many analogies to carotins ($C_{28}H_{38}$), which are so marked as to lead to the conclusion that it is itself a carotin and so possesses great interest as a colouring substance common to both the animal and vegetable kingdoms, and as an instance of the production of a hydrocarbon by animal agency.—On the intercellular substance, by M. Louis Mangin. It is shown that among Phanerogams and Cryptogams (with the exception of Fungi and many Algæ) the tissues of the softer parts are composed of cells cemented together by an intercellular substance composed of insoluble pectates.—On the localization of colouring-matters in the seminal integuments, by M. Louis Claudel.—Formation of quartz at the spring of Maubourot at Cauterets, by M. Beaugey.—On the existence of leucite rocks in Asia Minor, and on some hypersthene rocks from the Caucasus, by M. A. Lacroix. It is found that the leucitic rocks from near Trebizonde fall under two main types, leucitite and leucotiphrite.—Upon the composition of some pseudo-dolomitic chalks from the north of France, by M. L. Cayeux.

BERLIN.

Meteorological Society, January 7.—Dr. Vettin, President, in the chair.—Dr. Wagner spoke on the behaviour of water in the soil. The relationships between surface water and springs and deposits, possessing as they do a distinct meteorological interest, have as yet been but slightly investigated, probably because the behaviour of water in soil occupies the border-land between the subjects of meteorology, geology, agriculture, and hygiene. A review of scientific investigations which have so far been made on the subject of surface water and the formation of springs, shows that the problems of most importance are still awaiting their solution. In the speaker's opinion the task to be undertaken in the interests of meteorology is the establishing of as many lysimeters as possible, so that by keeping a continuous record of their indications a continued set of observations on surface water would be provided. He further considered it to be essential that the relationship of water to the soil should be investigated at depths far greater than has as yet been the case. A lengthy discussion followed the above communication, which turned chiefly upon a consideration of the forces, as yet but little known, which determine the collecting of water on internal impervious layers of the earth.—Prof. Spörer gave a short statistical statement on sun-spots during 1889. The chief point of interest was that the spots appeared during the first half of the year in the lower latitudes and in the second half in the higher. Taking the year as a whole, there were considerably more spots in the southern than in the northern hemisphere; this has been the case in each year since 1883.—The Secretary then handed in his annual report, and the Society proceeded to elect its officers for the year 1890. Prof. Schwalbe was elected President.

Physical Society, January 27.—Prof. Kundt, President, in the chair.—The President opened the meeting by a short address in memory of civil engineer G. A. Hirn, who died recently at Logelbach in Alsace.—Dr. Lehmann spoke on the testing of tuning-forks. After the International Congress met for the establishing of a uniform standard of tone, and selected for this purpose a vibration frequency of 435, it devolved upon Government to construct a standard fork, and to devise some ready method for testing ordinary forks to an accuracy within half a vibration per second, and standard forks within 0·1 of a vibration. The speaker discussed the various methods in use for comparing two forks and for counting the number of vibrations per second which they yield. For the first purpose the vibrations of the respective forks are employed, these being observed either acoustically or optically; a further means of effecting the comparison is by the stroboscopic method or by the acoustic wheel. The vibration frequency of a fork is determined either graphically or by means of a tuning-fork clock, or by means of the undulations obtained by oscillating or rotating acoustical instruments. An important factor in all these methods is the temperature of the fork. To determine this a special thermostat is employed, by means of which the fork can be set in vibration in an air-bath whose temperature is constant and accurately known. The standard fork for reference is one of König's, whose vibration-frequency has been accurately determined by several methods. The comparison of any new fork with the standard is made by means of the acoustic wheel, and by a simultaneous graphic recording of the movements of the fork which is vibrating inside the thermostat, and of the magnetic interrupter; the latter consists of a tuning-fork vibrating to the octave below the note yielded by the standard fork.—Dr. Eschenhagen exhibited curves of the three elements of terrestrial magnetism recorded by the new instruments in the Observatory of Potsdam, and gave a short description of the arrangement of the apparatus. The curves were taken on white photographic paper, and were of such dimensions that the greatest variations which have as yet been observed were completely recorded.—Prof. Kundt exhibited some quartz-fibres which he had received from Prof. Weinhold. He made, in addition, some remarks on the preparation of these fibres by Boys's method, and gave some data as to the dimensions of an apparatus which Prof. Weinhold had constructed for the measurement of gravitation constants, and had employed in several determinations.

AMSTERDAM.

Royal Academy of Sciences, Dec. 28, 1889.—Prof. van de Sande Bakhuyzen in the chair.—M. Hugo de Vries related the results of the scientific researches made by the Committee of Advice, appointed in July 1887 at Rotterdam, to report on the appearance of Crenothrix in the drinking-water of the Rotterdam water-supply. He gave an account of the organisms met with in the mains and basins before and after the filtration of the water, and of the degree of the pollution caused by these creatures in the colder and warmer months of the year. He spoke also of the influence of darkness on the water-organisms, which, under ordinary circumstances, live in the sunlight; of the

proposals made by the Committee to mitigate or remove the evil; and of the improvements effected, or about to be effected, in accordance with those suggestions.—M. Kapteijn treated of chronographical observations for the purpose of determining parallaxes of fixed stars. After having explained the precautions taken to prevent systematic error, he gave the results and subjected them to several tests showing their absolute trustworthiness within the limits defined by the probable errors.

Jan. 25.—Prof. van de Sande Bakhuyzen in the chair.—M. Hoogewerff, giving an account of joint work by himself and M. van Dorp, spoke of the action of potassium hypobromite on succinphenylamide, and on the amide of cinchonic acid.—M. van Bammelen communicated certain results of a research relating to the composition of volcanic and other soils, on which, in Deli and Java, tobacco is cultivated. The extraordinary fitness of the soil of the cleared forest grounds in Deli for the production of exquisite tobacco is to be attributed, he thinks, to the peculiar composition of the amorphous silicate occurring therein, to the looseness of the forest soil, and to the auspicious climate with regard to the rainfall. He concluded by insisting on the urgent need for the establishment of a scientific experimental station at Deli. Such an establishment would be favourable to the culture of tobacco, and would enlarge our knowledge of the soil, of the vegetable world, and of geological formations.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, FEBRUARY 20.

ROYAL SOCIETY, at 4.30.—A Comparative Study of Natural and Artificial Digestions (Preliminary Account): Dr. A. Sheridan Lea.—On a Fermentation causing the Separation of Cystin: Sheridan Delépine.—Some Stages in the Development of the Brain of Clupea harengus: Ernest W. L. Holt.

LINNEAN SOCIETY, at 8.—On the Fruit and Seed of Juglandia; on the Shape of the Oak-leaf; and on the Leaves of Viburnum; Sir John Lubbock, Bart., P.C., M.P., F.R.S.

CHEMICAL SOCIETY, at 8.—The Behaviour of the most Stable Oxides at High Temperatures: G. H. Bailey and W. B. Hopkins.—The Influence of Different Oxides on the Decomposition of Potassium Chlorate: G. J. Fowler and J. Grant.

ZOOLOGICAL SOCIETY, at 4.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 21.

GEOLOGICAL SOCIETY, at 3.—Annual General Meeting.

PHYSICAL SOCIETY, at 5.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.—The Geometrical Construction of Direct Reading Scales for Reflecting Instruments: A. P. Trotter.—A Paralle Motion Suitable for Recording-Instruments: A. P. Trotter.—On Bertrand's Refractometer: Prof. S. P. Thompson.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Some Types of American Locomotives, and their Construction: C. N. Goodall.

ROYAL INSTITUTION, at 9.—Magnetic Phenomena: Shelford Bidwell, F.R.S.

SATURDAY, FEBRUARY 22.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

SUNDAY, FEBRUARY 23.

SUNDAY LECTURE SOCIETY, at 4.—Our Ancestors, the Sea-Kings: Justin H. McCarthy, M.P.

MONDAY, FEBRUARY 24.

SOCIETY OF ARTS, at 8.—Stereotyping: Thomas Bolas.

TOYNBEE PHILOSOPHICAL SOCIETY, at 8.—Will and Reason: B. Bosanquet.

TUESDAY, FEBRUARY 25.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—Exhibition of Stanley's Spirometer: Dr. J. G. Garson.—Some Borneo Traps: S. B. J. Skertchly.—The Dieri and other Kindred Tribes of Central Australia: A. W. Howitt.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Shanghai Water-Works: J. W. Hart.—The Tytam Water-Works, Hong-Kong: Jas. Orange.—The Construction of the Yokohama Water-Works: J. H. T. Turner. (Discussion.)

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, FEBRUARY 26.

GEOLOGICAL SOCIETY, at 8.—On a Crocodilian Jaw from the Oxford Clay of Peterborough: R. Lydekker.—On the Relation of the Westleton Beds or "Pebbly Sands" of Suffolk to those of Norfolk, and on their Extension Inland; with some Observations on the Period of the Final Elevation and Denudation of the Weald and of the Thames Valley, Part III.: Prof. Joseph Prestwich, F.R.S.—On a Deep Channel of Drift in the Valley of the Cam, Essex: W. Whitaker, F.R.S.

SOCIETY OF ARTS, at 8.—The English in Florida: Arthur Montefiore.

THURSDAY, FEBRUARY 27.

ROYAL SOCIETY, at 4.30.

SOCIETY OF ARTS, at 5.—The Northern Shan States and the Burma-China Railway: William Sherriff.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 28.

AMATEUR SCIENTIFIC SOCIETY, at 8.—Practical Coal-mining: H. S. Streatfeild.

ROYAL INSTITUTION, at 9.—Evolution in Music: Prof. C. Hubert H. Parry.

SATURDAY, MARCH 1.

ESSEX FIELD CLUB, at 7.—Micro-Fungi of Epping Forest; how to Collect, Preserve, and Study Them: Dr. M. C. Cooke.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Elementary Dynamics of Particles and Solids: Prof. W. M. Hicks (Macmillan).—La Vie des Oiseaux: Baron D'Hamonville (Paris, J. B. Baillière).—A Naturalist's Voyage round the World, new edition, illustrated: C. Darwin (Murray).—A Naturalist among the Head Hunters: C. M. Woodford (Philip).—Geology of the Quicksilver Deposits of the Pacific Slope, and Atlas to accompany same: G. F. Becker (Washington).—Fossil Fishes and Fossil Plants of the Triassic Rocks of New Jersey and the Connecticut Valley: J. S. Newberry (Washington).—Il Teorema del Parallelogramma delle Forze Dimostrato Erroneo: G. Casazza (Brescia).—Materials for a Flora of the Malayan Peninsula: Dr. G. King (Calcutta).—Journal of Physiology, vol. xi. Nos. 1 and 2 (Cambridge).—Transactions of the Wagner Free Institute of Science of Philadelphia, vol. 2 (Philadelphia).—Observaciones Magnéticas y Meteorológicas del Real Colegio de Belen de la Comp. de Jesus en La Habana, Julio-Dic. 1887 (Habana).—Bulletin of the U.S. Geological Survey, Nos. 48 to 53 (Washington).—Department of Agriculture, Melbourne, Bulletin No. 4 (Melbourne).—"Timehri," being the Journal of the Royal Agricultural and Commercial Society of British Guiana, December 1889 (Stanford).

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